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Philosophy of Science

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The Impossibility of Interaction between Mind and Matter[1]

BY

E. GAVIOLA

INTRODUCTION

THE progress of psychology as scientific theory has been handicapped by the circumstance that it has been inclined to deal with two kinds of problems: on the one hand, with emotions, instincts, complexes, ideas, etc.; on the other, with the working of the sense-organs and of the central and sympathetic nervous system. To approach the first task it has had to create a system of mentalistic or introspective concepts, like the ones mentioned; to deal with the second enterprise it has adopted the physical language used by the natural sciences.

All science starts with the observation and the recording of facts. Then the facts are ordered and classified. For this, abstract concepts (logical constructs) have to be created. For example: a multitude of observational facts of behavior of a given animal is classified under the labels of a few instincts, simplifying thus the description of its acting under given circum-

[1] This paper owes its origin to a series of lectures given in 1933 at the "Colegio Libre de Estudios Superiores" in Buenos Aires, which were partially published in "Cursos y Conferencias, Año III, N. 4, p. 369 (1934)," Buenos Aires.

stances. The next step consists in the establishment of relations between the abstract concepts, with two purposes in mind: (1) to simplify the classification still further, by reducing concepts called secondary to the ones chosen as fundamental; (2) to be able to predict future facts of observation from the recorded past ones.

As soon as psychology tries to establish relations between physical and mentalistic concepts, fundamental difficulties arise: Is there an interaction between mind and matter? If so, where and how does it take place? If not, in what way are the two systems related?

The problem is very old, as old as philosophy itself. The best brains of the past have dealt with it and there would be little hope of adding anything new if a similar problem had not arisen in physics. The system of concepts used by this science was supposed, until recently, to be a single one, free of contradictions. But this century has discovered that a fundamental duality was hidden inside the apparently harmonious scientific building: the duality waves-corpuscles. The understanding of the character of this dualism in the realm of physical concepts, throws new light upon the old problem of the relationship between mind and matter. This is my excuse for writing this article.

OLDER THEORIES

Mind and matter form a conceptual unity for primitive people as well as for children. Material things and natural phenomena are supposed to be animated by spirits, or animae, in the same way as we are. These animae are material and mental at the same time and created according to our image and resemblance. No problem of interaction exists for primitive philosophies. The contemplation of a dead animal, particularly of a dead human being, introduces perhaps the first conceptual separation between mind and matter. There is a group of characteristics, shape, color, weight, etc., that we may call *static* qualities, which persist unchanged through death. Another group, that we may call dynamic qualities, changes greatly: the animal does not move after death and does not respond to external stimuli in the usual

way. If we call the static qualities matter and the dynamic ones, or the spirit or anima that is supposed to cause them, mind, we have created a dualistic theory of death. Mind and matter united constitute life; when they separate at death *we see* that matter persists, most ancient philosophies and nearly all religions *assume* that mind persists too.

The conceptual separation creates immediately the problem of interaction: In life, are mind and matter independent, autonomous, or interdependent? Our theory of life assumes that mind moves the body. Observation seems to show that matter (alcohol for instance) influences the mind.

It could be argued that the problem is artificial, since death can be assimilated to the going out of order of a purely material machine: a watch that stops, for instance. This argument was adequately answered by Descartes 300 years ago. I may doubt about the existence of mind, I may doubt also about the existence of matter, I may doubt about any and every thing; but who doubts? I doubt, ergo at least I exist as a doubting, thinking being: "cogito ergo sum." I may call this I that thinks my mind. If we admit further the existence of matter, we have the problem of interaction again. That the thinking mind, or consciousness, can not be explained away in terms of matter, as the materialistic doctrines pretended since Democritus (400 B.C.) until late in the last century, forgetting Descartes, was demonstrated again in 1872 by Du Bois Reymond in Germany.

Descartes, with his dualistic theory of reality, had to face the problem of interaction. But, curiously enough, he restricted his considerations to the investigation of *where* mind and matter hold their mutually influential reunions. After a careful study of the anatomy and physiology of the human body, he decided that the pineal gland is the meeting place. Other thinkers saw soon that the question of *where* was irrelevant as long as the problem of *how* was not solved.

THE IMPOSSIBILITY OF INTERACTION

In terms of the older philosophies the question can be answered in the following way: Only things of the same *essence* can have

any influence upon each other. Mind and matter are things of different essence. Therefore mind and matter can not have any influence upon each other.

The *essence of a thing* is called nowadays the *definition of a concept.* If I define psychical and physical concepts so that interaction is possible, interaction will exist; if I define them so as to exclude interaction, it will not take place. The question would appear to be thus a matter of pure conventionalism. But we can not create anew and arbitrarily the psychical and physical concepts. There is such a thing as pure psychology, that deals with concepts such as emotions, instincts, ideas, etc., and there is such a thing as physics, that deals with concepts such as mass, position, momentum, energy, etc. These concepts have been already defined according to principles of adequacy and economy of description. Our problem is to deal with them.

Now, matter obeys principles of conservation of mass plus energy, momenta and electric charges. Any influence upon matter implies the variation of at least one of these quantities. If one material system acts upon another, the changes of mass, momenta, energy and electric charges of the second are compensated by equal and contrary changes in the first. If mind is supposed to act upon matter, it would have to possess mass, energy, momenta or electric charges. But according to the usual psychological definition, it does not possess them. Therefore an action of mind upon matter is impossible. It would mean the breaking of the laws of physics.

Another way of coming to the same conclusion is the following. A material particle can be considered as the center of a gravitational and an electromagnetic field. Any influence upon it implies the modification of, at least, one of these fields. The modification can be brought about only by means of other fields of the same kind. Therefore, either mind possesses gravitational or electromagnetic fields, or it can have no influence upon matter. If mind has such fields it has also mass, energy, momenta and electric charges, obeys the laws of physics and has nothing to do with the mind psychology deals with.

A vitalist could answer that the interaction between mind and

matter occurs in living organisms and that the principles and laws of physics are not sufficient to explain their behavior. The foregoing proofs would not therefore be acceptable for living matter. I shall not consider here the arguments of the vitalistic school, since they have been already thoroughly dealt with by Philipp Frank.[2] Frank has shown that there is no proof that living matter obeys laws different from those of non-living matter, and that there is no indication that physico-chemical theories are insufficient to deal with the material phenomena of life.

We must conclude then, that interaction between mind and matter is impossible. This being so, philosophers have looked for a way of avoiding the question. I will consider briefly three such ways: materialism, spiritualism and psycho-physical parallelism.

Materialism asserts that matter is the only reality and that mind is nothing but the result of the functioning of organized matter. That this assumption is impotent to deal with the problems of consciousness was shown by Descartes and Du Bois Reymond, as has been mentioned already.

Spiritualism asserts (Leibniz, 1700) that everything is mind. This point of view would be acceptable if it were possible to build a system of psychical concepts capable of dealing with the phenomena of the natural sciences. But it has so far proved sterile before the problems of matter. Spiritualism must therefore be discarded on the principle of adequacy.

THE PSYCHO-PHYSICAL PARALLELISM OF SPINOZA

Spinoza saw clearly in 1677 that the Cartesian dualism had to be abandoned on account of the impossibility of interaction between mind and matter. He constucted the system called *monism*. There is one substance only, but it is unknowable for us. We can perceive two attributes, or modes, of that one substance: mind and matter, or as he called them *thought* (or consciousness) and *extension*. "Substance thinking and substance extended are one and the same substance, which is now compre-

[2] Philipp Frank, "Das Kausalgesetz und seine Grenzen," Springer, Wien, 1932.

hended under this attribute and now under that."[3] Mind and matter being two different attributes of one unknowable substance, there can be no interaction between them. The Cartesian problem seems therefore solved. But there must be, on the other hand, a precise parallelism between mind and matter. To each state of the one substance must correspond always two parallel attributes: one psychical, the other physical. Any explanation of a phenomenon in terms of physical concepts can be placed parallel to another one of the same phenomenon but in psychical terms. "Whether we think of Nature under the attribute of extension, or under the attribute of thought, or under any other attibute whatever, we shall discover one and the same order, or one and the same connection of causes; that is to say, in every case the same sequence of things" (l. c.).

Now, this necessary conclusion of Spinoza from his premises reduces his system *ad absurdum*. According to it, psychology should be parallel to physiology and their reasonings should follow one and the same order and one and the same connection of causes. But in this case psychology would be nothing but physiology translated into another scientific jargon. To each physical concept, to each word in the language of physiology, I could adjoin a mentalistic concept or a word of the psychological language; the result would be a bilingual dictionary. The difference between mind and matter would be analogous to the difference between *bread* and *Brot*. The statement of Spinoza, supporting an analogous one of Descartes, that the essential difference between mind and matter consists in the fact that the first is indivisible while the second is not, contradicts his psychophysical parallelism. If the statement were true, there could not be "one and the same order" in both.

Spinoza's theory would lead to the introduction of a psychical fourth (or fifth?) dimension for each material particle. An attempt to describe the contents of consciousness in terms of the "minds" of the electrons that form the body would prove certainly inadequate and uneconomical.

[3] The Philosophy of Spinosa, by Joseph Ratner, p. 160, Tudor 1926–7.

Discarding the brilliant and subtle theory of Spinoza, we have not proceeded beyond a recognition of the impossibility of interaction between mind and matter. But now the discovery of a similar fundamental duality within the system of physical concepts used in the natural sciences throws new light upon the old problem.

THE DUALITY WAVE-CORPUSCLE[4]

Physicists thought, until the beginning of this century, that the concepts they used were all of "one and the same essence," and that they could therefore interact. When they began to study *how* waves of light interact with matter composed of corpuscles they discovered that such interaction is impossible. Einstein created therefore in 1905 a corpuscular theory of light, thus making light of "the same essence" as matter. Later on (1925), Schrödinger created undulatory theory of the atom, making matter of "the same essence" as light. We thus have two different and complete systems of concepts each of which is potentially capable of covering the whole field of physics. Now, are the two systems parallel? I will consider an example in order to make the answer to this question clear: the phenomenon of resonance radiation, discovered by R. W. Wood in 1911.

Let us assume we have a sealed glass bulb, emptied of air, containing sodium vapor at an appropriate density. The bulb is illuminated by the concentrated radiation of a sodium lamp (a light source which emits the yellow lines of sodium). Observation shows that the vapor absorbs and reemits the yellow lines of sodium in all directions. The secondary light is called the resonance radiation.

I can explain this phenomenon in two different ways. According to one system of explanation, the incident light is constituted by photons, or corpuscles of radiation. When these photons collide with the corpuscular atoms of the vapor, they are absorbed, and the absorbing atoms pass from one stationary state of energy into another one. In this last state, the atoms stay a

[4] A detailed discussion of this duality has been published in "Contribución al Estudio de las Ciencias, Univ. de La Plata, Vol. 5, p. 245, (1931).

short casual time and then return to the original stationary state, emitting a photon of yellow sodium light in a casual direction. In this description I want to call attention to the facts that (1) an atom either absorbs a whole photon or nothing, (2) the relative number of atoms that take part in the process is small, under ordinary conditions less than one in a million, and (3) the intensity of the resonance radiation is proportional to the average number of atoms that absorb and reemit light. According to the other system of explanation, the incident light is constituted by waves; the atoms themselves are of a wave nature, and I can symbolize them by harmonic oscillators. When the light waves reach the atoms, the oscillators increase their amplitude, absorbing part of the incident light and emitting spherical coherent waves. In this explanation the points to be noticed are: (1) an atom may absorb any arbitrarily small quantity of light energy, and this quantity may vary continuously, (2) all the atoms illuminated by the primary light, practically all the atoms in the bulb, take part in the process, and (3) the intensity of the resonance radiation is proportional to the square of the average increase of the oscillator's amplitude.

The two descriptions explain the same phenomenon, each in a complete and satisfactory way. But is there a parallelism between the two? The answer is no. Compare the corresponding facts to which attention was called in the two explanations. Atoms that according to the first description take no part in the process, "ignore" it, play a rôle in the second explanation.

Another way of showing the absence of parallelism is to notice that to a single corpuscle corresponds in the undulatory system a great number, a "package," of waves; while to a single monochromatic wave a great number of corpuscles has to be related.

A relation between the two systems of concepts, undulatory and corpuscular, exists, but it is purely statistical, not causal. There is no causal interaction between waves and corpuscles.

The question may be asked why, if the two systems are equivalent, one does not displace the other, leaving physics with a single set of concepts of the same "essence," as undoubtedly would be desirable. The answer is that if one of the systems

could explain all the facts that the other does and a good number of them in an equally or more economical way, it would displace the other. But this is not the case. Corpuscles can not explain interference, on the one hand, and waves have not, so far, described satisfactorily the observed tracks in a Wilson chamber, on the other. Both systems therefore have to be retained.

CONCLUSIONS

The foregoing example shows clearly that the naïve question about what light is *in reality*, wave or corpuscle, is void of logical sense. Waves and corpuscles are neither real nor unreal, they are creations of the human mind that serve the purpose of describing and predicting phenomena. They may be adequate or inadequate; the theories built with them may be true or false, but not real or unreal.

The only reality is the Cartesian primary fact: my existence as a consciousness that thinks. The immediate facts of my experience are constituted by the contents of my consciousness: a sequence of impressions, emotions, representations. The purpose of science (including philosophy) is to order and classify the contents of my consciousness, with the ultimate aim of being able to predict future immediate facts of experience from past ones. For that purpose, I construct systems of concepts (labels of classification) under which I order my impressions, emotions, etc. Then I construct a set of logical rules that permit me to correlate the concepts. The only restriction upon the number of different systems of concepts that I may create and upon the sort of logical rules that I may use is my desire for success in my predictions and for economy of effort in my classifications and theories. In the development of science two main systems of concepts have been created. One of them constitutes what is called the physical world, the other what may be called the world of the mind or the psychical world.

To eliminate contradictions in the first system, it has been necessary to divide it into two complete and different physical worlds: the corpuscular and the undulatory. The natural sciences deal with these two physical worlds. Psychology *should* deal

with the world of the mind. In the last century, however, it has dealt mostly with a chapter of the natural sciences: the physiology of the sense organs and the central and sympathetic nervous systems. This curious fact has been brought about, probably, by the enormous progress of the natural sciences, and by the hindrances put in the way of the science of the mind by religious and moral prejudices. The impetuous development of psychoanalysis in the last two decades indicates that psychology is finding its way back to its proper field of work. In the meantime, it is common to find theories that contain physiological and mentalistic concepts mixed together. Since the two systems of concepts have been defined so that interaction between them is impossible, the mixture of them creates an internal conflict that will lead to paradoxes and contradictions. To avoid the latter, a theory has to stay within a single system of concepts of the same "essence." I may perhaps quote Spinoza again: "So that when things are considered as modes of thought, we must explain the order of the whole of Nature or the connection of causes by the attribute of thought alone, and when things are considered as modes of extension, the order of the whole of Nature must be explained through the attribute of extension alone. . . ." (l. c.)

The sentence "alcohol influences my mind" is certainly logical nonsense, since there is no interaction between mind and matter. The natural sciences had to create a mathematical language and a mathematical logic to avoid senseless sentences in their own field. Psychology will have to fulfill a similar task, to exclude contradictory phrases, like the one mentioned, in the realm of the mental sciences.

I wish to acknowledge my gratitude to Dr. Donald C. Williams, who read the manuscript of this paper and made many helpful suggestions.

University of Buenos Aires, Fac. of Sciences.

On an Empirical Criterion of Meaning

BY

HORACE S. FRIES

IN VIEW of the importance of the philosophical method argued in Mr. C. I. Lewis's "Experience and Meaning,"[1] I wish to call attention to ambiguities which may have an important bearing on one of his conclusions. The method for which he argues is a certain empirical test of meaningfulness. It is his 'positivistic' inference from this method which I wish to challenge. To do so I shall present four points: (1) A summary of his empirical test of meaningfulness; (2) A 'non-positivistic' (or 'dualistic') hypothesis which Mr. Lewis rejects; (3) An analysis of this hypothesis in view of his test; (4) The theory of universals implied in my analysis.

(1) In order for meaning to be empirically tested it must be verifiable. But (pp. 141–142) the term 'verifiable' is ambiguous. "On the one hand this connotes a certain character of the content of one's assertion or hypothesis. *This must be envisaged in sensuous terms* [my italics]; it must be the case that we could recognize certain empirical eventualities as verifying it, supposing that the conditions of such verifying experience could be satisfied. Verifiability in this sense requires an empirical *content* of the hypothesis, but has nothing to do with the practical or even the theoretical difficulties of verification. . . .

[1] *Proceedings and Addresses of The American Philosophical Association 1933*, Vol. VII of the combined proceedings and addresses of the several divisions. Reprinted from *The Philosophical Review*, Vol. XLIII, No. 2, March, 1934; p. 125. Page references below refer to this paper.

"On the other hand, 'verifiable' connotes the possibility of actually satisfying the conditions of verification. Or . . . verifiability may be taken to require 'possible experience' *as conditioned by the actual;* we must be able to find our way, step by step, from where we actually stand to this verifying experience. Hence practical or theoretical difficulties are limitations of verifiability in this second sense." But these last are limitations to knowledge, not to meaningfulness.

In this passage I have emphasized the sentence "*This* [content of the hypothesis] *must be envisaged in sensuous terms.*" For it seems to me to contain an important ambiguity which Mr. Lewis neglects. In one sense of the term 'sensuous terms' the 'positivistic' inference is valid. In this sense the term means *bare immediate particulars* considered independently of the *structured* or *universal* aspect of most, if not all, sensuous experience. In the second sense the term means the structured or universal aspect of a sensuous experience. It is this sense of the term that makes a 'non-positivistic' inference valid.

(2) The 'non-positivistic' theory of meaning which I wish to analyze is this: The term 'meaning,' used in the same sense, I believe in which Mr. Lewis uses it, refers to a relation of 'reference' between the 'datum,' a sensuous experience, and the 'cognoscendum' to which the datum refers. The 'non-positivistic' or 'dualistic' theory to be analyzed holds that *in some cases* the cognoscendum may be such that it cannot under any circumstances be a datum. Mr. Lewis rejects this theory (p. 135) on the grounds "that a relation of experience to what cannot be brought within experience is a relation which cannot be investigated, and one the very conception of which as cognitive involves a confusion of thought."

This quotation seems to me to contain another ambiguous term which Mr. Lewis neglects. For the term 'experience' may refer to immediate experience or to mediate experience. I shall try to show that this distinction is not without a difference when I examine it in view of Mr. Lewis's criterion and when I examine the implied theory of universals. In the meantime we may note that the 'dualisitc' theory I am defending does not neces-

sarily hold that the reference relation is *never* within immediate experience.

Now if instances of the reference relation occur within immediate experience, it must be within an immediate experience which is present at once. For if the datum occurs now in my present immediate experience and the cognoscendum later in my immediate experience or in the immediate experience of someone else, then the relation is not entirely within immediate experience. Mr. Lewis is not clear on this point (pp. 132, 134, 136). But if we must have an instance of the relation within immediate experience in order to *envisage* it, then he must allow such cases. But if it does not occur within immediate experience, then it can only be envisaged by analogy from similar relations which do occur within immediate experience. I believe the theory of universals implied in the analysis which follows allows for this exigency. But I believe the reference relation does occur within immediate experience. It would be a peculiar situation if we could not have ideas present about our present immediate experience. An example of the presence of the reference relation seems to me to be my present idea (one aspect of which is the datum) of the white paper I now immediately experience, where the white is part of the cognoscendum.

(3) In another place[2] I have argued that the 'dualistic' theory now to be analyzed is the least incompatible with the findings and methods of the sciences. But here I wish merely to defend it against Mr. Lewis's rejection of it on the grounds that it is incompatible with an empirical test of meaningfulness.

Let us be on our guard against over-simplification. The perceptual and epistemological situations are in all probability rather complex. At least they are complex to discourse about because of the vagueness and ambiguity of their vocabulary. This second complexity results in part from the fact that there *appears* to be two distinct kinds of phenomena involved in perception and knowledge. One is causal and the other is contemplative or that of spectating (the immediate presence in consciousness). In the

[2] "On the Spatial Location of Sensa," *Phil. Rev.*, XLIV, 345–353.

paper referred to above I have argued that an idea (as distinguished from its symbol) cannot ordinarily be present at once in consciousness. It seems to me that having an idea means to have a certain causal structure which goes far beyond the conscious specious present. It goes beyond it in time as well as in the unconscious spatial brain or muscular-nervous system. But this aspect of the problem need only be mentioned here, since my concern is primarily with the cognoscendum.

The cognoscendum can be referred to by a single term. This term may be envisaged as one term of a diadic relation. But this is no sign that the cognoscendum is a term of a simple diadic relation. For even a single term is not sufficient for reference until thought or communication about the cognoscendum has occurred.

Let us set up an imaginary situation which corresponds to the 'dualistic' theory I wish to defend. Let the cognoscendum be a relational complex, *some* of the terms of which cannot be within immediate experience and some of which can be within some future immediate experience.

I shall use the term 'idea' to mean the complex experience which *refers to* the cognoscendum. The term thus may include so-called thoughts *and* some sense data. Our problem is (1) to show how that part of the cognoscendum which cannot be within immediate experience may be envisaged so that it is verifiable in the first sense of the term, or meaningful and (2) to show how we can find our way actually step by step to an experience which verifies the idea. We shall take the second problem first and in a simplified form.

Let our idea, i, refer to cognoscendum, c. c has two terms, c_1 and c_2. i attempts to 'reflect' c_1 and c_2. The more adequately i actually reflects c_1 and c_2 the truer is i. We shall call these reflections r_1 and r_2. c_1 is incapable of being in any immediate experience. But r_1 attempts to envisage it. (How, we are to see later.) r_2 is the *anticipated* consequence (in immediate experience) of the experience or experiment which will test i. c_2 is the *actual* consequence (in immediate experience) of the experience or experiment which either verifies or fails to verify

i. r_2 intends to reflect c_2. If it does so, then c_2 turns out to be r_2. But if *i* is verifiable (in the second sense) it must be constructed (thought) so that r_2 is implied by r_1; i.e., if r_1 then r_2. If r_2 is adequate to c_2 then r_1 also implies c_2. If also r_1 is adequate to c_1 then c_1 implies r_2 and c_2. If c_2 turns out to be r_2 then *i* has been verified.

But, for two reasons, although *i* has been verified, it may not be true. For (1) r_1 may have been inadequate to c_1, and (2) affirming the consequent of an implicative relation does not necessarily affirm the antecedent; at best it adds to its probability. If c_2 turns out not to be r_2, then *i* is unverified. This means either (1) that r_2 was not implied by r_1. In this case a deductive mistake was made in constructing *i*. *i* may still be true or false as far as the experimenter knows. Or (2) if r_1 *really implies* r_2, then *i* is probably false. For if the consequent of an implication relation is denied, so is the antecedent. In this case *i* cannot be true *unless* r_1 (which intended to reflect c_1) 'refracts' c_1 in such a unique way that the error in the intended reflection exactly compensates for the discrepancy between c_2 and r_2.

This unique coincidence is, of course, unlikely. Nevertheless a *crucial* experiment is required to prove that *i* is false. But a crucial experiment is possible only when the experimenter is entertaining incompatible hypotheses. The question as to whether or not r_1 *really implies* r_2 is too complex to be treated briefly. But this question is a part of the question as to the adequate reflection of c_1 by r_1. When we set up a crucial experiment we have two hypotheses each of which is more or less compatible with the body of knowledge. If the experiment is properly set up; i.e., if the ideas are properly constructed, the results are decisive in proportion to the adequacy of our body of knowledge. Revolutionary experiments require a fundamental reconstruction of the entire body of relevant knowledge—e.g., Einstein.

But before we construct the logical outline of a crucial situation we must consider more carefully the non-crucial situation above when c_2 turns out to be r_2 and thus verifies *i*. If *i* is then true (or to the degree it is true) r_1 was an adequate reflection of c_1.

But since c_1 can never have been within immediate experience, how can we know that r_1 is adequate to it? For example, I might predict that since devils cause storms and since devils come out on nights such as tonight, tomorrow will probably see a storm. Tomorrow the storm comes, and my idea is verified. The reason why my idea about devils is taken to be false is that it does not fit in with the rest of our knowledge. I must try to construct i so as to be in harmony with other knowledge or else (e.g., Einstein) I must be able so to shift the structure of the body of knowledge that it harmonizes with my idea, i. The ideal 'fitting in' is of course the relation of implication; we aim to formulate deductive systems. But knowledge begins by lucky gropings in the dark. As it grows it more adequately reflects the structure of the innumerable c_1'*s* which constitute that aspect of experience (or Reality if you like) which can never be within immediate conscious experience. The actual sensuous or imaginative envisagement of such realities may change or vary considerably without changing the *structure* of the innumerable r_1'*s*. It is the *structural* reflection which counts. As physics becomes more and more exact, its ideas become more and more like bare structures, symbols without much sensuous content. But it is more exact because the structure of its r_1'*s* has become more adequate to the structure of its c_1'*s* as tested by the correspondence between its r_2'*s* and its c_2'*s*. It is important to remember that physical knowledge is an abstraction from our body of knowledge and that the structure of the physical world is therefore only part of the structure of the world at large. By the time all knowledge becomes deducible from physical knowledge, the body of physical knowledge and other bodies (e.g., psychology, biology, etc.) will have undergone many Einsteinian revolutions.

Let us now very briefly construct a crucial situation. We might do so in terms of one idea or of two ideas. The latter will simplify our symbolism.

Let i be one idea and $\bar{\imath}$ be another. Both intend to reflect c_1. But in i, r_1 (let us say ir_1) is different from r_1 in $\bar{\imath}$ ($\bar{\imath}r_1$). The general intention of the idea upon verification is to eliminate either ir_1 or $\bar{\imath}r_1$ as the reflection of c_1. ir_1 implies ir_2. $\bar{\imath}r_1$ implies $\bar{\imath}r_2$.

But to be crucial i and $\bar{\imath}$ must be constructed so that ir_2 and $\bar{\imath}r_2$ are related as contraries. That is, if ir_2 then not $\bar{\imath}r_2$; this, of course, implies if $\bar{\imath}r_2$ then not ir_2.

If, upon verification, c_2 turns out to be ir_2, then ir_1 is verified and may be true, but $\bar{\imath}r_1$ is eliminated. And if c_2 turns out to be $\bar{\imath}r_2$, then $\bar{\imath}r_1$ is verified, and ir_2 is eliminated. The uncertainty element, as in the non-crucial situation, is introduced because of our doubts as to the adequacy of the reflection of c_1 in r_1.

We may now turn to the question of the envisagement of c_1, that aspect of the cognoscendum which can not be within immediate experience. According to the dualistic theory, we cannot know what kind of qualities, if any, are embodied in the events which constitute c_1. But for prediction and control of (i.e., knowledge about) our immediate experiences, past, present, and future, our concern is with the *structure* of the external world. The sensuous elements in our immediate experience by means of which we envisage the structures of external events serve merely as some of the terms of the *structures* of our ideas; and it is the structural reflection of the external world rather than the sensuous terms of this structure which is important for knowledge *about* our immediate experience; i.e., for our knowledge about how to predict and control it. Whether knowledge *about* the *intrinsic* nature of our immediate experience—e.g., redness, pleasure, displeasure, hardness, etc.—is to be classified with knowledge *about* its causes and consequences seems to me to be merely a matter of convenient classification at present. The fact remains, however, that we do make comparisons of intrinsic natures; one thing is a brighter color than another or more pleasant.

Now the possibility of reflecting the structure of external events in our ideas and of making comparisons of the intrinsic natures of our immediate experiences can be accounted for by one and the same theory of universals. We now turn to a brief survey of this theory.

(4) In the treatment above of the cognoscendum, it was taken to be a universal; i.e., something which could recur. According to the theory of events, which seems to have replaced the old theory of substance in the philosophy of the sciences, an event

does not recur. Rather it is an occurrence which when it happens is done with forever. But this difficulty is easily avoided if we take the idea of similarity seriously and use it to explain apparent identities or recurrences, instead of trying to explain 'apparent similarities' as cases of identity.

Let us examine the hypothesis, then, that universals are simply relations of similarity. Without defining 'similarity' logically (Russell and many others have done so), let us illustrate it as it may occur between relations, sets of relations (structures or classes), mere qualities, and the classes of these things (since it is convenient to distinguish between mere classes and classes which are structures). There are degrees of similarity. If we take Cohen's principle of polarity seriously, it is difficult to see how we can assume literal identities, since this would then imply absolute dissimilarity.

If *f* is more similar to *g* than to *h*, the similarity between *f* and *g* is a different relation from that between *f* and *h;* and both relations are similar. If in a certain respect, *f* is similar to *g* exactly as *f* is similar to *h*, I again assume the relations to be numerically different but exactly similar. Or take exact similarity between two qualities, say, two instances of red in a certain respect. The universal in the situation is a relation of exact similarity; it is not redness. Exact similarities might be said to be identical but numerically distinct; but such a statement might be misleading. For the assumption that exact similarity presupposes an identical subsistent or existent factor present in the similar instances is unverifiable and unnecessary. For suppose that there were such an identical factor in the case of exact similarity. By definition it could not be distinguished from *mere* exact similarity by any conceivable kind of inspection. For its presence (or existence) could not make any difference which could not be assigned to properties of *merely* exactly similar instances. It is thus a purely *ad hoc* assumption and is not required by a philosophy of events.

With this theory of universals we can see how the occurrence of the 'same idea' may be an occurrence of a similar idea. If the similarity is not sufficient, thinking and communication will be confused. The degree of similarity required will depend on the purposes in hand. For the purposes of formal (symbolic) logic

there is almost no limit, practically, to the degree of similarity that may be obtained as long as our thinking sticks to its *fiats:* its indefinables, definitions, and postulates—in short, its resolutions.

But for physics or for any applied logic (or applied mathematics, of course) something else legislates for our thinking besides our fiats. This legislative factor is the coming and going of our immediate experiences "*as conditioned by the actual.*" Even our formal fiats are conditioned by the actual psychological conditions which obtain, but in applied realms there is another actual factor, which is extra-psychological. These actuals we call physical events. But according to our theory of universals the relations (structures) among these events may be more or less similar to our ideas about them. Hence our ideas about them may more or less adequately reflect these structures. It is therefore irrelevant to the fact of knowledge whether or not all of these structures *can* literally occur within immediate experience. Even Mr. Lewis, since he rejects traditional positivism, would not hold that all events *do* occur within some immediate experience. For what is required for knowledge is a sufficient degree of similarity between our ideas and their objects and a means of testing this similarity. In so far as our immediate experiences are determined (directly or indirectly) by these external structures we have a means of verifying, but merely with probability, the adequacy of our reflections of these structures, providing we construct our ideas properly—i.e., providing we take account of relevant factors in the body of knowledge.

May I close with one word of warning? To maintain that our ideas reflect an external structure is not to maintain that this structure is exhaustive or unchanging and eternal. Perhaps Reality is bottomless; perhaps even change itself is not absolute but is relative and changing. Perhaps, then, man must construct for himself whatever degree of permanence and security he may desire which is now wanting in that aspect of Reality which manifests itself in human experience.[3]

Lawrence College,
Appleton, Wis.

[3] Cf. M. C. Otto, "Meditation on a Hill," *Phil. Rev.*, XXXIX (1930), 329–350.

A Note on the Fruitfulness of Deduction

BY

LEO ABRAHAM

DEDUCTION has frequently been condemned as a useless intellectual instrument because of its tautological character.[1] To thoroughgoing opponents of rationalism, the pretensions of deduction are on the same level with those of induction. Both presume to yield *more* knowledge from the fact that we have some knowledge;[2] and this is an impossible paradox, which not even so powerful an opponent of the "Philosophy of Experience" as Bradley could resolve to his own satisfaction.[3] I wish in this brief note to indicate how this semblance of paradox may very easily be removed without having to deny, as has often been thought necessary, the tautologicality of deduction.

If we admit that deductive inference is tautological or analytic in character, it can hardly be doubted that we are bound to accept the view that the conclusion of an inference is contained in the premisses and cannot be novel with respect to all of them. All distinctions between the logical and the psychological,[4] the

[1] *E.g.*, for three modern examples of this condemnation, *cf.* L. Wittgenstein, *Tractatus Logico-Philosophicus*, 6.1 *ff.*; D. W. Prall, "Implication," *University of California Publications in Philosophy*, 1931, pp. 127 *ff.*; and F. C. S. Schiller, "*Is* Socrates Mortal?," *Mind*, 1935, pp. 204 *ff.*

[2] Poincaré, *Foundations of Science*, p. 155; N. Campbell, *Physics: The Elements*, p. 161.

[3] *Principles of Logic*, second edition, vol. II, p. 599.

[4] *E.g.*, M. R. Cohen, "The Subject Matter of Formal Logic," *Journal of Philosophy*, 1918, p. 681.

constitutive and the epistemic,[5] seem to me unavailing at this point. For if inference is analytic, the conclusion must, by definition, be part of the premisses; and if we are fully aware of all our premisses, we must be equally aware of our conclusion. Should the conclusion of an inference strike us as novel, it only follows that we have not been fully aware of all its premisses, and have therefore not actually performed a valid inference.

How is it, then, that deduction does seem to bring new information? How is it that one apparently tries to prove by deduction the apparently startling fact that deduction can be never startling? It seems to me that there are two explanations of this phenomenon.

The first explanation is linguistic. Some of the novelty of deduction appears to be due to the nature of language. One fact or proposition may be expressed in many different symbols, not precisely identical in all their meanings; and those tautologies whose conclusions are represented by language which does not exactly repeat the language of one or more of their premisses are regarded as more significant than those expressed in repetitious language.[6]

From the point of view of the *propositions* or *referends* involved in inference, however, it is clear that such linguistic novelty is quite beside the mark. Another explanation, therefore, is required in order to show how a more genuine novelty is possible in deduction. This explanation rests on a very simple distinction. While the conclusion of a deductive inference cannot be new with respect to *all* its premisses, it is perfectly possible for it to be new with respect to *any* or *some* of them—or even *every one* of them considered singly.[7] In view of the fact that deductive inference,

[5] W. E. Johnson, *Logic*, Part II, Chapter I.

[6] Thus, the classical syllogism in Barbara, "All men are mortal; Socrates is a man; therefore Socrates is mortal," has an appearance of non-tautologicality because the collective term 'all' obscures the presence of the *proposition* 'Socrates is mortal' in the major premiss. Similarly, when it is inferred that something is colored because it is red, the use of two different terms, 'colored' and 'red,' hides the tautologicality of the inference.

[7] Symbolically expressed, if a proposition s can be deduced from propositions p, q and r, s *may* be novel with respect to either p, or q, or r, or pq, or pr, or qr; but it cannot be novel with respect to pqr. In general, where n is the number of premisses, there seems to be $2^n - 2$ ways in which the conclusion of a deduction *may* be novel with respect to its premisses.

as a temporal process occurring in an individual, involves the consideration of a *succession* of more or less distinct propositions, it is accordingly perfectly comprehensible how what is known at the end of the process may be considerably more than what is known at its beginning. Thus, if we have lost our way in some unfamiliar city, and if, by combining our rudimentary knowledge of the principles of cartography with an examination of a street map of the city and the sign post at the nearest corner, we manage to deduce our precise whereabouts, we have acquired new information—in the perfectly unimpeachable sense that we now know something of which a few minutes before we were ignorant. Similarly, in court of law, in the mystery melodrama of fiction and the cinema, and even in the normal routine of everyday life, the piecemeal accumulation of relevant bits of information may point inexorably to a most unforeseen conclusion.

The novelty of any such conclusion cannot be reduced to the novelty produced by empirical observation. There are innumerable instances of deduction in which the purely empirical premisses, examined singly, give not the slightest intimation of their possibly breath-taking consequences, considered jointly.[8] Since all that need be meant by 'deduction' is just this joint consideration of associated propositions,[9] it follows that deduction and learning are not mutually exclusive categories. The error in the anti-rationalist's position is the fallaciousness of his argument from the true proposition that to know all the premisses of a deductive inference is to know the conclusion as well, to the false proposition that *therefore* deduction cannot extend our knowledge. Super-rationalists not infrequently make the converse mistake of arguing from the indisputable fact that deduction does produce new knowledge to the erroneous belief that *therefore* to know all the premisses of a deductive inference is not also to know the conclusion. Both errors have an identical root—

[8] "Take the classical example: A priest says: 'My first penitent was a murderer, and a baron soon thereafter entering the room says, 'I was the reverend father's first penitent.' The dramatic conclusion is surely not contained in either premiss" (M. R. Cohen, *Reason and Nature*, p. 194).

[9] *Cf.* C. S. Peirce, *Collected Papers*, vol. II, p. 387.

an implicit theory of deduction as not merely the combination of premisses according to certain rules, but as some manner of mental leap from the combined premisses to a separate and distinct conclusion. This belief makes the anti-rationalist overlook the fact that the deductive extension of our knowledge proceeds simply by the combination of premisses; and it leads the super-rationalist to ignore the obvious truth that when our premisses are combined we therewith have and know our conclusion.

To summarize, the 'paradox' that the conclusion of an inference can apparently be both certain and novel, deductive and yet productive, is to be explained by the consideration that while its novelty is respective to any one premiss, or to any group of its premisses short of all of them, its certainty is dependent upon its premisses being taken together in their entirety. There is thus no incompatability between the analytic or tautological character of deductive inference and its usefulness as a means of extending our knowledge.

The Johns Hopkins University.

Are Some Propositions Neither True Nor False?

BY

CHARLES A. BAYLIS

THOUGH some doubts about the principle that every proposition is either true or false were entertained even by Aristotle, both the number and the vigor of criticisms of this principle have been increasing in recent years. This paper attempts a restatement and a re-examination of the issues involved in this dispute, and in particular an evaluation of the effects on the argument of such recent discoveries as that of the "many-valued logics."

There has been a tendency in some quarters to regard the existence of the "many-valued logics" as definite grounds for the rejection of the principle of excluded middle. Thus, for example, Professor E. T. Bell asserts that up to the time of the discovery of Łukasiewicz and Tarski "It was still supposed that a consistent workable system of deductive reasoning must follow the Aristotelian pattern, particularly the second law as applied to statements: a statement is either true or false. If such were indeed the case, then it would seem to follow that human beings in their 'search for truth' could never shake off the tyranny of these laws, and in particular they must always be subject to the second law. . . . To destroy this most obstinate of all superhuman Absolutes it would suffice to produce a workable, consistent (not self-contradictory) system of deductive reasoning in which the

second law is invalid. This was done in 1930 by Łukasiewicz and Tarski."[1]

An examination of the facts, however, reveals clearly that the existence of the "many-valued logics" is quite irrelevant to the truth or the falsity of the principle of excluded middle. To be sure, since each of these n-valued systems is defined by the matrix method it follows that their laws are tautologies and have that special kind of necessary truth that tautologies possess. But this is just as much the case when n has the value 2 as when it has the value 3 or 5 or aleph$_0$. There is no contradiction between the systems. All the laws of the two-valued, and of the three-valued, and of the n-valued systems are necessarily true. For each law is so framed that it covers all cases possible in the system and is thus true no matter what possibility is actualized.

But, as C. I. Lewis well puts it,[2] though these laws are necessarily true, their significance depends upon what they are true *of*. For the systems in themselves are not systems of logic at all. They are abstract structures whose "values" may be any set of predicates of the specified number which together are exhaustive of the possibilities, whose elements are any entities such that at least one (and usually, also, at most one) of these predicates may correctly be asserted of any given element, and whose relations have the properties specified by the defining matrix. The laws of the system are necessarily true for any values, elements, and relations which meet these conditions. Whether there are any such or not may be a question. Whether there are any which will make of the system a logic is a further question. In any case, the correctness of any definite proposal to the effect that certain specific predicates, entities, and relations meet these conditions must be tested, not by the logic of the abstract system, for it is not a logic unless a valid interpretation of its abstract constituents exists which makes it a logic, but by an antecedently known logic whose nature is involved in the nature of the predicates, entities, and relations proposed.

[1] *The Search for Truth*, Baltimore, The Williams & Wilkins Co., 1934; pp. 254–255.

[2] *Symbolic Logic*, by C. I. Lewis and C. H. Langford, New York, the Century Co., 1932; p. 222.

Consider, for example, the two-valued system of Łukasiewicz and Tarski, which is equivalent to the two-valued system of *Principia Mathematica*. In itself this is an abstract structure whose constituents are the values 0 and 1, the elements, p, q, r, *etc.*, and the relations N and C (in *Principia*, $\sim$ and $\supset$). Can we find an interpretation of this structure such that, as interpreted, it will be a logic? The usual one proposed is that the elements, p, q, r, *etc.* may be regarded as propositions, the values 0 and 1 as falsity and truth, and the relations N and C as negation and implication, respectively. Is this a correct interpretation? Nothing in the abstract structure itself is sufficient to tell us. We must ask other questions the answers to which depend upon our antecedent logical understanding of the nature of propositions, truth, falsity, implication, and negation, *etc.* Thus we must ask: Are truth and falsity correctly predicable of propositions? Is at least one of them predicable of any proposition? Only if both these questions may be answered affirmatively is it correct to regard the system as one of logic dealing with the truth-relations of propositions. If it may be so regarded then we may be sure that the laws of the system are necessarily true of certain relations between propositions. Just what these relations are, in turn, can be determined only by a study of the relational properties specified by the matrix definitions. The relations defined by the matrix may be ones with which we are already familiar and for which we have names, or they may be relations to which we have not yet been introduced. In the case of the system in question, the defined properties of N correspond very closely to what we mean by negation, while the properties of C correspond only in part to what we have ordinarily meant by implication so that we distinguish this relation by the special name "material implication."

Should we, on the other hand, find that some propositions are neither true nor false, but have some third value, then we could not properly regard the two-valued calculus as subject to an interpretation that would make it a truth-value logic of propositions. If we believe that it requires three truth-values to exhaust the truth-value possibilities of propositions, we should need to

take the three-valued calculus; if we believe that there are five truth-values we should need to take the five-valued calculus, and so on, before we should find one which could properly be interpreted as a truth-value logic. We must know first whether or not the principle of excluded middle is true before we can decide which of the n-valued abstract systems is exemplified by our truth-value logic of propositions. The existence of these n-valued systems does assure us that no matter how many truth-values we find propositions capable of, we shall have at hand an abstract calculus for the ready elucidation of the tautologous laws of a truth-value logic. It does not help us at all to determine whether there are two, three, or many truth-values. In so far as Professor Bell and others[3] urge that it does they seem to be indulging in that "unbridled popular scientific theorizing" that Professor Bell so rightly deprecates.[4] Whether the principle of excluded middle is true or false is a question the answer to which depends on considerations quite other than those derived from the discovery or elaboration of abstract mathematical structures.

Łukasiewicz, in his account of the philosophical significance of his many-valued systems[5] recognizes this and offers his alternative systems, not as necessitating rejection of what he calls the principle of two-valuedness, but as furnishing a framework for a valid logic should it be found necessary on other grounds to reject this principle. He calls attention to Aristotle's discussion of the applicability of the principle to propositions about the future,[6] and says that this led him to the following reflections:

[3] Even so careful a writer as H. Margenau, in a discussion ("On the Application of Many-Valued Systems of Logic to Physics," *Philosophy of Science*, Vol. 1, No. 1, Jan. 1934, p. 119) in which he rightly opposes the much more radical suggestion of F. Zwicky that the application of the many-valued logics to physical reasoning gives rise to a "principle of flexibility of scientific truth," feels constrained to admit: "Many-valued logic admits of a series of truth values including the equivalents of true and false, and perhaps including doubtful. This means that every proposition is either true, or false, or doubtful."

[4] E. T. Bell, *op. cit.*, p. 273.

[5] "Philosophische Bemerkungen zu mehrwertigen Systemen des Aussagenkalküls" in *Comptes Rendus des Séances de la Société des Sciences et des Lettres de Varsovie* (*Sprawozdania z pozesiedeń Towarzystwa Naukowego Warszawskiego*), Classe III, Vol. XXIII, 1930, Fascicule 1–3, pp. 51–77.

[6] *Op. cit.*, pp. 64, 75.

"I can assume without contradiction that my presence in Warsaw at a definite moment of time of next year, for example, noon of the 21st of December, is today decided neither in a positive nor in a negative sense. It is thus *possible* but *not necessary* that I shall be in Warsaw at the given time. On this assumption, the assertion, 'I shall be in Warsaw at noon on the 21st of December next year,' can today be neither true nor false. For if it were today true then my future presence in Warsaw would have to be necessary, and this contradicts the assumption; and if it were today false, then my future presence in Warsaw would have to be impossible, and this also contradicts the assumption. The proposition under consideration is thus today *neither true nor false* and must have a third value different from '0' or the false and from '1' or the true. This value we can denote by '½.' It is precisely 'the possible,' which as a third value takes its place alongside of 'the false' and 'the true.'

"It is to this course of thought that the three-valued propositional calculus owes its origin."[7]

Łukasiewicz is quite correct in his judgment that the many-valued systems do not dictate the rejection (or acceptance) of the principle of two-valuedness, but that, on the contrary, the acceptance or rejection of this principle is one of the determinants of the form of abstract system which will be acceptable as a logic. He seems wrong, however, in urging that unless his presence

[7] "Ich kann ohne Widerspruch annehmen, dass meine Anwesenheit in Warschau in einem bestimmten Zeitmoment des nächsten Jahres, z. B. mittags den 21 Dezember, heutzutage weder im positiven noch im negativen Sinne entschieden ist. Es ist somit *möglich*, aber *nicht notwendig*, dass ich zur angegebenen Zeit in Warschau anwesend sein werde. Unter dieser Voraussetzung kann die Aussage: „ich werde mittags den 21 Dezember nächsten Jahres in Warschau anwesend sein," heutzutage weder wahr noch falsch sein. Denn wäre sie heutzutage wahr, so müsste meine zukünftige Anwesenheit in Warschau notwendig sein, was der Voraussetzung widerspricht; und wäre sie heutzutage falsch, so müsste meine zukünftige Anwesenheit in Warschau unmöglich sein, was ebenfalls der Voraussetzung widerspricht. Der betrachtete Satz ist daher heutzutage *weder wahr noch falsch* und muss einen dritten, von „0" oder dem Falschen und von „1" oder dem Wahren verschiedenen Wert haben. Diesen Wert können wir mit „½" bezeichnen; es ist eben das „Mögliche," das als dritter Wert neben „das Falsche" und „das Wahre" an die Seite tritt.

"Diesem Gedankengang verdankt das dreiwertige System des Aussagenkalküls seine Entstehung." *Op. cit.*, p. 64; translation mine.

or absence in Warsaw at a given future time is now determined[8] it is today neither true nor false that he will be there then.

The critical points of his argument are the inferences: (a) from "If it were today *true* that P" to "P would have to be *necessary*," and (b) from "If it were today *false* that P" to "P would have to be impossible."[9] Łukasiewicz offers no justification whatever for these inferences and they seem pretty clearly invalid. There are three considerations which may have led him to make such inferential leaps, but none of them offers any real justification. (1) The inferences would be valid were truth identified with necessary truth and falsity with necessary falsity. But no such identification is justifiable without at least the assumption of a complete determinism of the kind that Łukasiewicz specifically rejects at the beginning of his argument. And even such a deterministic assumption would confer only causal and not logical necessity.

(2) Łukasiewicz, following certain suggestions of Aristotle, may have reasoned in this way: "If it is true that I shall be in Warsaw at a given time it can not be the case that I shall not be there then. But if it is impossible that I shall not be there then, it is necessary that I shall be there then." Such reasoning, however, is also invalid. For what is impossible is not that "I shall not be there then," but that "I both shall and shall not be there then." The conjunction is impossible, but neither conjunct is. On the assumption that it is factually (*i.e.*, not necessarily) true that "I shall be there then," it follows with necessity

[8] Łukasiewicz speaks of his future presence in Warsaw as not now "decided" (*entschieden*). He might conceivably mean by his sentence either: (a) "I can assume without contradiction that my presence in Warsaw . . . is today *determined* in neither a positive nor a negative sense," or (b) "I can assume without contradiction that today it neither *is* nor *is not the case* that I shall be in Warsaw . . . " If he meant (b) he would be begging the whole point at issue and his argument which follows would be entirely superfluous. It is more charitable to assume that he meant (a). That he did mean (a) is indicated also by his statement two paragraphs earlier that "The dispute about the principle of two-valuedness has a metaphysical background: the advocates of this principle are resolute determinists while the opponents of the principle incline to an indeterministic Weltanschauung."

[9] "P" is used here as an arbitrary abbreviation for "I shall be in Warsaw at noon on the 21st of December of next year."

that it is factually false that "I shall not be there then." It does not follow that it is necessarily false that "I shall not be there then." In other words, because it is impossible that "P is true and P is false," it necessarily follows from the assumption that "P is true" that "P is not false," but it does not follow that P's falsity is impossible or P's truth is necessary.

(3) Łukasiewicz may base his inferences on the assumption that a proposition can not today be true unless there is today some fact in virtue of which its truth is necessitated. This assumption, however, if made with regard to temporal propositions is false. For the truth of propositions to the effect that certain events did occur in the past it is necessary only that the occurrence of these events was at the past time specified a fact, and, similarly, for the truth of propositions to the effect that certain events will occur at a given time in the future it is necessary only that the occurrence of these events at that time will be a fact.

Consider, for example, a proposition about the past, say the proposition, "Columbus discovered America in 1492." Is it not sufficient for the present truth of this proposition that in 1492 it was a fact that Columbus was discovering America? Surely his discovery of America need not be a fact today. It may well be that in order that today we should *know* that Columbus discovered America in 1492 it is necessary that we be acquainted with some fact which would justify the inference that in 1492 it was a fact that Columbus was discovering America. But the existence of such a fact and of our acquaintance with it is necessary not for the *truth* of "Columbus discovered America in 1492," but for our *knowledge* of that truth. To be sure, if this be a deterministic world of a certain sort there will today be facts from which we might justifiably infer that a past fact corresponding to a given true proposition about the past did exist. But even if this world be not such a deterministic one, it may nevertheless be true that "Columbus discovered America in 1492."

The case is similar with regard to propositions about the future. What is needed in order that the proposition,

"Łukasiewicz will be in Warsaw at noon on the 21st of December of next year," should today be true is only that at the time specified it will be a fact that he will be there. Whether there are now any facts which determine the facts which will be at that time is another question, a question the answer to which will be relevant to the truth or falsity of determinism. Unless there are such facts and unless they can be known, *knowledge* of the future may well be impossible. But such facts are not essential to the *truth* of propositions about the future.

Neglect of this distinction between knowledge of truth and truth seems to be at the base of another series of attacks upon the principle of excluded middle, namely, the attacks of certain of the mathematical finitists such as Brouwer. They urge that certain propositions, such as "All numbers of the form $2^{2^n+9} + 1$ are factorable," are such that: (a) they are not known to be true and are not known to be false, and (b) there is no available method for determining whether they are true or false, and (c) they *are* neither true nor false. The only evidence they advance for (c) consists of propositions of the type of (a) and (b), and it is clear that if the usual distinction is made between truth and knowledge of truth, (a) and (b) are not sufficient to prove (c).

Miss Alice Ambrose, following a suggestion of Weyl, has urged that finitists could better make their point by denying that such verbal forms as "All numbers of the form $2^{2^n+9} + 1$ are factorable" are propositions at all rather than by attacking the principle of excluded middle.[10] The finitist position seems to rest upon insistence on an operational theory of meaning and the further insistence that the defining operations be at least theoretically performable. Thus, since they hold that a one by one examination of an infinite set is not possible, their objection to verbal forms such as the above is, or at least consistently might be, that the operation necessary to give them meaning is unperformable and that, hence, they are meaningless and do not signify propositions. If this interpretation of the finitist's position correctly represents what they mean to say, their view does not involve

[10] Alice Ambrose, "Finitism in Mathematics," *Mind*, Vol. XLIV, No. 174, April 1935, pp. 186–203; and No. 175, July 1935, pp. 317–340.

the denial of the principle of excluded middle. If, however, anyone should urge that such verbal forms as the above do have meaning but that the propositions they signify are neither true nor false, he could properly be asked to produce better proof of the latter part of his assertion than the statement that no performable method of proving them true or false is known. For such a statement could be grounds for that assertion only if truth were identified with verifiability.

Of course it may be that some who deny that every proposition is either true or false do so not because they confuse truth and verifiability but because they wish to use the word "truth" to signify what is ordinarily signified by the word "verifiability." With such persons there seems little need to dispute, for what they propose is only a terminological change and they could not object logically to the use of some other word for what is now signified by the word "truth." Such a terminological change can at most increase or decrease somewhat the suitability of our language for the transmission of meaning or feeling, and its advisability seems a linguistic and not a philosophical problem.

Much more important than such terminological considerations are such questions as the following. Are there sets of concepts, other than the truth-falsity set, such that every proposition exemplifies at least one member of the set and that the concepts of the set together exhaust the relevant possibilities? If there are such sets, are they more useful than the truth-falsity set? Can any of them serve as the "values" for a matrix logic?

That there are a number of such exhaustive concept sets is apparent. One, very important for some purposes, is the epistemic trichotomy of "known to be true," "known to be false," and "not known to be either." Since rational human action requires knowledge, such a trichotomous classification of propositions may well be more useful for general human needs than the objective true-false dichotomy. The three-valued calculus of Łukasiewicz and Tarski is precisely interpretable as a logic of propositions with these three values, and this fact makes possible the ready elucidation of certain of the relations which hold between propositions of these three types.

There are many other exhaustive classifications. Thus the

class of propositions the truth value of which is unknown in turn may be divided into two sub-classes, those where the truth value is unknown but is knowable, and those where the truth value is not only unknown but unknowable. This distinction would be of particular importance to anyone trying to discover the limits of possible knowledge. Or, again, propositions could be classified as necessary, true but not necessary, false but not impossible, and impossible. Then again, it might be desirable for certain purposes to distinguish various kinds of necessary propositions, as, for instance, those of the form, "ab implies a," where the necessity is due to a subsumptive relation, and those of the form "a or not a" where the necessity is due to an exhaustion of the possibilities.

For any such exhaustive classification of propositions into n classes the work of Łukasiewicz and Tarski has provided us with an n-valued matrix calculus for the precise statement of certain relations between members of these various classes. Hence, it may well be that for certain purposes the classification of propositions into 3 or 4 or 5 or n classes with the use of the appropriate 3 or 4 or 5 or n-valued calculus will yield more useful results than the usual classification into true and false propositions. There is nothing sacred about the true-false classification, and in those cases where another proves more useful there is no reason why we should not make use of it.

It needs to be emphasized, however, that the availability of other classifications and of matrix systems to fit them is no evidence against the exhaustiveness of the two-valued classification. That this is the case is evidenced by the always present possibility of formal cross-classification. Thus the classifications (a) true, false, and (b) known to be true, known to be false, not known to be either, when combined result in the following classification:

(1) true and known to be true
(2) true and known to be false
(3) true and not known to be either
(4) false and known to be true
(5) false and known to be false
(6) false and not known to be either

Here classes (2) and (4) are each internally contradictory and hence empty, but this means only that, in order for the two-valued principle of division to be exhaustive, it must be the case that any proposition will fall in at least one of the remaining classes and thus be either true, whether known to be so or not, or false, whether known to be so or not. What would be needed to show that some propositions are neither true nor false is not merely the possibility of trichotomous or other exhaustive divisions but a demonstration that some propositions which are not known to be true and are not known to be false are in fact neither true nor false, and this is a demonstration which has yet to be given.

In short, recent discoveries in logic and mathematics are irrelevant to the truth of the principle of excluded middle. For that principle to be true it is necessary only that the division of propositions into true and false should be exhaustive, and whether it is or not depends on the meaning of the terms, "true," "false," and "proposition." These meanings are no doubt not entirely clear, but that they are such as to make the division exhaustive is one of the most certain things about them. The division is intended to be a dichotomy. To assert that a proposition is false is to assert that it is not true, and to assert that it is not true is to assert that it is false. And, contrariwise, to assert that a proposition is true is to assert that it is not false, and to assert that it is not false is to assert that it is true. This much at least we intend by these terms, whatever else their intension may include. The only adequate grounds for abandoning this intent would be that it is an inconsistent one. Absence of evidence that this is the case is absence of any reason for abandoning the principle, which follows from the nature of these concepts, that every proposition is either true or false, the principle of excluded middle.

Department of Philosophy,
Brown University,
Providence, R. I.

The Structure of the Proposition and the Fact

BY

ALBERT WOHLSTETTER

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FOR the most part, this paper[1] is an attempt to understand such a statement as this: "The proposition represents the fact by virtue of a structural identity between it and the fact." The statement is a familiar and important one in modern philosophy. It is, for example, one of the tenets of Logical Positivism.

[1] In preparing this paper for publication, I owe much to the criticism of my friend Mr. Lawrence Kagan.

The formulation of this problem involved an analysis and in some cases the redefinition of the following words: "similarity," "proposition," "propositional form," "operational meaning," and "fact." This analysis is as interesting and important as the central problem.

I. STRUCTURE

1. The sense of "structure" in which it is significant to say that two different situations or, rather, relations have the same structure has been defined by Russell. His version is convenient.

The *structure or "relation-number* of a given relation is the class of all those relations that are similar to the given relation." It is to be noted that the structure of a *relation* is defined. In this usage only a relation or a system of relations can have a relation-number. From this abstract point of view, a structure is a class—a class of "similar" relations. All relations belonging to that class (that is, having the same structure) are similar. Thus, to say that two relations have an identical structure is the same as saying that they are "similar."

Russell's is an analytic definition which presupposes the definitions of "similarity," "domain of a relation," "converse-domain," "field," and "one-one relation." I will define the last four terms and then define "similar" in terms of these.

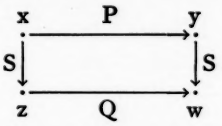
Briefly, if R is a relation, its *domain* is the class of all those terms which have R to something, the class of referents; its *converse-domain* is the class of all those terms to which something has R, the class of relata; and the *field* is the combination of both the domain and the converse-domain, the sum of the referents and the relata. If x R y, then the domain is the class of all those terms which can satisfy x, the converse-domain is the class of terms satisfying y. Take the relation "father of," x father of y. The class of fathers is the domain. All the children are the converse-domain and the field is the class of all the children and fathers.

A relation is one-one where the selection of the referent uniquely determines the selection of the relatum and conversely. A few examples should make this clear. "X is married to one

spouse Y" is one-one. If Mr. Lowe is substituted for X, Mrs. Lowe inevitably is the value for Y, and vice-versa. "X is the sovereign of Y" is one-many, not one-one. Take *Noel Coward* as a value of Y and X must be *King Edward.* On the other hand the substitution of "King Edward" for X does not uniquely determine the value of Y. Any British subject can serve as a value for the variable. Another instance of a one-one relation is "twin-of."

These preliminaries enable us to define relation-likeness precisely.

Two relations P and Q are *similar* "when there is a one-one relation S whose domain is the field of P and whose converse-domain is the field of Q, and which is such that if one term has the relation P to another, the correlate of the one has the relation Q to the correlate of the other and vice-versa." We can draw a map of this relation of similarity.



P is similar to Q when $xSz \cdot zQw \cdot w\breve{S}y$

P is similar to $Q \underset{df}{=} S \mid Q \mid \breve{S}$

Three things to be noted are that (a) isomorphic relations have the same logical properties, (b) structure is independent of the nature of the terms, (c) similarity is independent of the nature of the relations.

1.1. The standard illustration of isomorphism is the geographical map. The map of New York State has the same structure as *New York.* For every round black dot on the map there is a town on the country side (one to one correspondence) and the relations between the dots are relationally similar to the relations of the towns. That is to say, whenever the relation "below" holds between two points on the map, the correlative towns will be related by "South of" and so forth.

2. The definition of structure as a *class* of relations may be

objected to, as too abstract. The objection is beside the point. As a matter of fact it is not necessary for this discussion to define structure in this way, or, in *any* way. We are not interested in the structure of a relation taken in isolation, but in the case where two relations have an identical structure. We are interested not so much in structure as isomorphism. Two relations are isomorphic when they are similar. Thus relational similarity is the concept important for this paper.

3. I want to distinguish three sorts of likeness. The first, relational similarity, has been treated in section one; similarity of classes, the second, has been defined by Cantor; and the third, qualitative similarity, is, I believe, undefinable.[2]

Two *classes* are similar when there is a one-one relation whose domain is the one class and whose converse domain is the other. This type of similarity, we shall see, is referred to loosely by some of the writers who advance the thesis examined, as isomorphism. However it is important not to confuse it with relational similarity, which is more than one-to-one correspondence. When two classes are similar they have the same cardinal number. When two relations are similar they have the same relation number.

The third type of likeness, qualitative similarity, seems to me to be very important. It requires more analysis than I have time for, and I know of no treatment of it to which the reader may be referred. This section is an attempt at a very partial analysis.

The sentence obviously doesn't resemble the fact it expresses in any usual sense of the word resemblance. Francis Bradley has had occasion sadly to point this out. The sentence "Caesar is pink" is very different from the blood and bones reality of the fact that *Caesar is pink.*

This "usual" sense of resemblance is what I mean by "qualitative similarity." Consider the case of these three situations.

[2] This was my belief two years ago. My present belief involves a change but it is not essential to the main thesis. "Undefinable" was used loosely. For in one important sense of "definition," clearly no term is intrinsically undefinable: definability is a systematic question. I was thinking of traditional definition *per genus et differentiam.* In this sense, perhaps, there are terms which are undefinable—those terms which are not species of wider genera, the widest terms. But I am not even sure of this.

1. A is on B; 2. C is on D; 3. E is to the right of F. It is possible that all three situations have the same structure, but 1. and 2. resemble each other in a very different sense from that in which they both may be said to be similar (relationally) to 3. 1. and 2. are surely more than relasimilar.

The taste of Phillip's Magnesia is *sensuously* similar to that of a good grade of white chalk. There is a qualitative similarity in their colors too. Taste, however, and color, as immediate non-cognitive experience, have no structure. Again, the consequences of placing your head in the fire are qualitatively similar to the consequences of placing it in an electric oven. Two chairs and a photograph of one of them may be relationally similar, but you can sit on the chairs and not on the image.

To avoid confusion we shall use the word "qualisimilar" for sensuous resemblance and "relasimilar" for isomorphism. Thus we may parallel the definition of "structure" as a class of relasimilar relations, with an arbitrary use of the word "essence" to signify a class of qualisimilar qualities or relations. Thus when two qualities or relations are qualisimilar they have a common essence. When two relations are relasimilar they have the same structure or relation number.

Now when I say that two terms have a common essence, I do not mean that there is some entity which they possess in common, and *in virtue of which they are qualisimilar.* I mean just that they are qualisimilar, and since I mean just that, the italicised phrase is nonsense. "Essence," here, is a verbal convenience not a hypostatic entity. Thus to say that these two patches both have redness, I believe, is the same as saying that if certain operations are performed on the first patch under certain lighting conditions and so forth, a certain quale will result, and that if qualisimilar operations are performed on the second patch the consequent color will be qualisimilar. The aspect of similarity will be determined by the operations selected—tactual, visual, aural, etc.

These definitions may be applied to the case stated above of three situations: 1. A is on B; 2. C is on D; 3. E is to the right of F. 1, 2 and 3 have the same structure. 1 and 2 have a common essence, and are essentially different from 3.

Qualisimilarity however needs further analysis. It is probably undefinable since it is one of the most universal traits of existence. And it is a very different sort of concept from that of relasimilarity. It seems to admit of degrees (in the sense that this donkey is more like that donkey than that zebra).

The distinction between "qualisimilar" and "relasimilar" seems bound up with a consideration of the nature of analysis. A non-relational term (that is a quality unanalyzed) can be only qualisimilar. Consider the case of the two donkeys and the zebra. Obviously each of these terms can be analyzed into terms in relation. Let us say that the arrangement of the parts of donkey one is relasimilar to the relations holding between the parts of donkey two and zebra one. It is also qualisimilar. The parts, as qualities, are *sensuously* similar or dissimilar. These parts of course may be analyzed further into parts in relation and again relasimilarity is possible. But at each stage of the analysis there are terms in relation and our distinction still holds. The non-relational terms at most are qualisimilar, the relations relasimilar and possibly qualisimilar. Thus simplism is not necessary for the validity of our distinction.

3.1. These distinctions may be applied to the standard instance of the geographical map—specifically to *two* charts of the same territory. Again all three—the two charts and the countryside—are structurally the same. Whenever town A is South of town B, dot A′ will be below dot B′ on map one, and dot A″ will be below dot B″ on map two. And again it is clear that the two maps are essentially identical with each other, and that they both differ in essence from the countryside. The relations *South of* and *below* are defined in terms of qualitatively unlike operations and consequences.

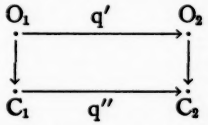
3.2. A statement in these terms of an operational treatment of knowledge may be clarifying.

From an instrumental view such as John Dewey's a judgment like "this is sweet" means "If I bite this, such and such an event will result." The "such and such" indicates that an event of a class of *qualitatively similar* events will occur. Awareness of this qualitative similarity would, I believe, not be knowledge.[3]

[3] Cf. Dewey, Quest for Certainty, p. 137.

For on the operational view, knowledge is the perception of a relation between the set of operations and the consequences. This has been said[4] in this way: "Knowledge is the recognition of a structure not given in immediate experience." In my terms, however, knowledge is not the recognition of a structure but of an *identity* of structure. What follows, an analysis of the relation between the operations and the consequences, makes this clear.

There is a relation of qualisimilarity, q′, holding between the operations, and another relation of qualisimilarity, q″, holding between their consequences. Qualisimilar operations have approximately qualisimilar consequences. It is possible then that q′ is isomorphic with q″. That is, there might be a relation of relasimilarity holding between the two relations of qualisimilarity, q′ and q″. Whenever q′ holds between O_1 and O_2 then q″ will



hold between C_1 and C_2. (O = operation; C = consequence).

S, the ordinal correlator, would, of course, be the causal relationship. (Plato's ideas would seem to be hypostatizations of of q's and q″s). This formulation is almost certainly oversimplified. However it does map an identity of structure (precisely defined) between the operations and their results.

4. Now I would like to compare my use of the phrase "identical in structure" as "relasimilar," with Wittgenstein's use of the same phrase. However I do not pretend to understand Mr. Wittgenstein very well and I find his discussion of structure especially difficult.[5]

Mr. Wittgenstein indicates with comparative clarity that he thinks there is a one-to-one correspondence between the elements of the proposition and the elements of the fact (Tractatus: 2.13, 2.131, 4.04). This is, of course, not enough for relational similarity. But he says further (3.21) "To the con-

[4] By Ernest Nagel in Lecture.

[5] No one has called Mr. Wittgenstein the Mystery Man of Modern Philosophy. I have no explanation to offer for this.

figuration of the simple signs in the propositional sign corresponds the configuration of the objects in the state of affairs." The nature of the "correspondence" of these configurations *may* be interpreted as relasimilarity.

Proposition 2.15 is important for Wittgenstein's view of structure.

"2.15. That the elements of the picture are combined with one another in a definite way, represents that the things are so combined with one another. This connexion of the elements of the picture is called its structure. . . ." Mr. Wittgenstein says that the connection of the elements of the picture "represents that the things are *so* combined." What does "so" mean here? In what sense are these two combinations the same? The relation of the elements of the picture is not *essentially* the same as the relation of the elements of the fact. They *may* be relasimilar. The notes of the musical score are ordered spatially by "to the right of." The sounds of the music as played are ordered by the relation "following in time." These combinations are essentially different but relasimilar. "So" is ambiguous.

In the light of this, Mr. Wittgenstein's description of structure as the "connexion of the elements of the picture" seems dubious. I can assign four meanings to it, in the phrase "identity of structure." The first would make the phrase nonsense, the second would make many of the propositions of the Tractatus in which the phrase occurs false, the third would confuse isomorphism with classimilarity, and the fourth would, properly, treat isomorphism and relasimilarity as synonymous. (1.) If the structure of the picture means literally the relation, here and now, in all its *particularity* between the elements, then it cannot be held in common with the pictured. The actual occasion of the relation just is, is not shared with anything, is identical only with itself. (2.) If it is the *mode* of combination, the *nature* of the relation, then isomorphism between the picture and the pictured does not necessarily hold. By "mode" and "nature" I mean, again, essence. As has been pointed out the relation "above" may be correlated with an essentially different relation "North of." (3.) and (4.) I believe that either the third or the fourth

alternatives suggested must be adopted if Mr. Wittgenstein's statement is neither meaningless nor false. And it seems to me he intended the third. However, I hope to consider both these interpretations of structure in my examination of the general thesis.

5. A distinction is usually made[6] between showing and representing a form. It is claimed that the structure of a proposition or of a fact is *not* represented in the sense of being named, but that it is *shown*. It shows a "form" in the sense that it "has" a form. Thus the structure of the fact isn't named but the same structure, it is said, is shown by the proposition.

So Wittgenstein—2.172: "The picture, however, cannot represent its form of representation, it shows it forth." and 4.0312: "My fundamental thought is that the 'logical constants' do not represent. That the logic of the fact cannot be represented." This may be translated into our terms. The proposition cannot say anything about the structure of the fact. The proposition *is* relasimilar to it. The meaning of this oracle needs to be examined. It might conceivably offer an excuse for an unprecise use of the word "structure." Mr. Wittgenstein bases his mysticism on the fact that structure cannot be talked about. The realm of the mystical is the inexpressible—that which cannot be said but, shown. In this realm Mr. Wittgenstein includes logic, philosophy and ethics.

5.1. The structure which this paper must show is that of the relation holding between the structure of the proposition and the structure of the fact.

II. THE PROPOSITION

1. Isomorphism has been defined precisely and illustrated. However just what it means to say that the *proposition* and the *fact* are isomorphic is not clear. In order to make it clear it would be necessary to specify the relations and its terms in the "prop-

[6] *Wittgenstein:* Tractatus 4.0312, 4.041, 4.12; *Wisdom:* "Logical Constructions;" *Stebbing:* Mod. Introd. Log., p. 127; *Russell:* passim. Mr. Wittgenstein however is not always consistent. See his article on "Logical Form" in the Proceedings of the Aristotelian Society, Supplem., Vol. IX.

osition," and the corresponding relation and its terms in the fact. Now in the case of the proposition at least, it is not at all easy to make these distinctions. The proposition is a very elusive entity at best. It is hard enough to establish its existence, much less actually distinguish its parts.[7] I had intended including such an analysis of the proposition (a sort of spectral analysis, I suppose), but it is a task to which I feel unequal. At any rate, it is necessary to try to understand what it is that is asserted to have the same structure as the fact.

2. In recent logic texts[8] the proposition, conventionally, has been distinguished (a) from the sentence "in all its particularity"—from the series of marks here and now; (b) from the fact which it expresses, the fact being "the locus of verification of the proposition"; and (c) from the judgment considered as a mental act. (I think we may take it for granted that the proposition is not the psychological act of judging.) But usually in these texts the proposition enters as a "*third thing*," a new entity wedged between the sentence and the fact. We have the sentence which expresses, not a fact, but a proposition, which in turn expresses the fact—which is an aspect of an event. On this view, it may be seen, there is a good deal of distance between the sentence and the situation it refers to.

The proposition thus, is reified. It is regarded as "what the words mean," yet, somehow, not the fact. It is the objective meaning of the sentence.[9] Or again it is "the object of thought

[7] See Mr. Ryle's very lucid paper in the Proc. Arist. Society, Vol. 30, called "Are There Propositions?" Mr. Ryle decides that there aren't any propositions. If we accept this analysis our task may be considerably simplified. For it would then be easy to demonstrate that all propositions are isomorphic with the facts represented, since, as all students of the class algebra know, the null class is included in any class. And so we might end here. The proof is rigorous but I feel that the reader may not be entirely satisfied. I shall go on.

[8] *Stebbing:* Mod. Introd. Log., Ch. IV & P. 115; *Cohen & Nagel:* ILSM, Ch. II; *Eaton:* General Log., Part I; *Mace:* Principles of Logic, Ch. III. I do not discuss Mr. Mace's theory or the proposition as the "objective factor in the judgment," since Mr. Mace *says* that he is oversimplifying the matter and indicates that he might accept the interpretation of the proposition as a logical construction.

[9] *Eaton*, op. cit., p. 12; *Stebbing*, p. 32 & p. 115; *Cohen & Nagel*, pp. 27 & 28. See *Ryle*, op. cit., pp. 100 & 101.

considered as a transitive act," but purified of all mental traits. It is not the *cogitatio* but the cogitatum.[10]

In this way the proposition is endowed with a sort of Platonic being.[11] I have no time to consider the difficulties that arise from this hypostasis. It seems to me that it solves nothing, merely reduplicates the problem. I would agree with Mr. Ryle that the theory is "neither plausible nor necessary." And it is only fair to point out that each of the authors mentioned have elsewhere denied the existence of such entities. Ralph Eaton in his "Symbolism and Truth" criticized the theory sharply.[12] Dr. Nagel, too, in his lectures. And Miss Stebbing, I am told, very recently in one of her many answers to H. W. B. Joseph. The texts are introductory and the proposition may have been platonized, for pedagogical reasons. But if we do assume this third thing, the difficulties as to the nature of what we are assuming still remain. As does the problem of discerning its parts and their relations.

3. Lately there have been a good many attempts to avoid this thingification of the proposition. Many logicians have realized that the proposition must be linked definitely with the symbol. Charles Peirce anticipated[13] this in the following definition.

The proposition is "a dicent symbol that is a legisign."

By a "*dicent*" sign Peirce means one that is taken as a sign of "an actually existent." A "*sinsign*" is a particular "existent thing or event which is a sign." Such that when "the" is written several times:—"the...the...the...the"—there are several different sinsigns. And a "legisign," according to Peirce, is a

[10] *Eaton*, p. 22; See *Ryle*, pp. 92–96. The theory of course is not modern. It goes back to Plato.

[11] Dr. Nagel seems to advance a slightly different theory also, on pp. 28 & 29 of I.L.S.M. "For propositions are at most only the abstract and selected relations between things. When we affirm or deny the proposition THE MOON IS NEARER TO THE EARTH THAN THE SUN, neither the moon alone, nor the earth, nor the sun, nor the spatial distance between them is the proposition. The proposition is the relation asserted to hold between them." On this view the proposition is equated with what W. E. Johnson calls the "Tie" of the fact. How this can be said to represent the fact or be true or false, I don't know. And I don't see how it is compatible with Dr. Nagel's views as stated recently in "Verifiability, Truth, and Verification," Jl. of Philos., vol. 31, p. 142: "When true, a proposition states relations between processes, but never *is* them."

[12] *Symbolism and Truth*, Ch. V.

[13] Collected Papers, Vol. 2, Bk. II.

general "law," a "type," of which sinsigns are instances or "replicas." (The law is established usually by men.) Thus when we write: "the... the... the... the," we have four different sinsigns which are all replicas of the same legisign; four sinsigns and *one* legisign. "Symbols" are signs which refer to the objects they denote "by virtue of a law, usually an association of general ideas" (all conventional signs). The sentence in all its particularity is then a sinsign—a replica of the proposition which is a legisign.

Now Peirce's treatment of the proposition emphasizes very properly the symbolic aspect. It seems to avoid both the hypostasis ("Its identification with a substantial, non-temporal, non-spatial entity or essence"), and the psychologizing of the proposition.[14] But I believe that there are difficulties. For one thing the status of the legisign is very unclear. It appears often as if a quite substantial entity has been smuggled in again. The notion of types and "replicas" is unsatisfactory. At best it is unanalyzed. (If we are to take "replica" seriously, there are of course the ancient difficulties as to whether or not the "type" is an instance of itself and other difficulties of the sort. But "replica" I suppose is pickwickian.) If the legisign is not to be taken as "a substantial, non-temporal, non-spatial entity or essence," it seems to me that an operational interpretation is needed.

When we say that two sinsigns (e.g. "the" and "the"), are instances of the same type or legisign, it doesn't seem to me that we are saying anything more than that they are qualisimilar. No entity held in common is necessary. When we say a sinsign has "theness" we can mean only that it is qualisimilar to, perhaps, some possible standardsin sign—"the" ("the" is qualisimilar to "the" but not to "and"). And to say we proceed according to the same rule in constructing these sinsigns is the same as saying that our procedures are qualisimilar. The legisign can therefore be treated as the relation of qualisimilarity between the sinsigns, or, better, as *the class of qualisimilar sinsigns*. Such that when we say that two sinsigns are replicas of the same

[14] Nagel: "Peirce's Guesses at the Riddle." Jl. of Philos., Vol. 30, p. 375.

legisign, what we mean is that they are members of the same class of qualisimilar sinsigns. And this last notion, which may seem too abstract, is just longhand for saying that the two sinsigns are qualisimilar.

And then there is another difficulty which Peirce's unanalyzed concept of the legisign does not, I believe, explain. It is easy to see how two qualisimilar series of marks, like "cogito" and "cogito" are instances of the same legisign. But how can "cogito" and "cogito" and "je pense" and "I think" all be said to be replicas of the same type? The four of them do not all *look* alike. Now it is important to remember that the procedure selected as the standard procedure of constructing the sinsigns is arbitrary (it is a symbol). We may say then, using Peirce's terminology, that the legisign is not just one "law" as typified by, say, "cogito" and "cogito," but the logical disjunction of all the patterns that have been thus selected arbitrarily. In denotational terms, two sentences (sinsigns) may be said to "express" (i.e. be members of) the same proposition if they are both qualisimilar to this standard object—"cogito," *or* this standard object—"je pense," *or* this—"I think," and so forth. Consider the easy case of the simple symbol. In order to be "instances of" the same legisign, two simple sinsigns, such as "the" and "le" do not have to be qualisimilar to each other, but both of them must be qualisimilar to:

le .V. the .V. el .V. il .V. ——

The standard with which the sinsign is to be compared is any one of the number of alternative series of marks established by the different conventions. It is the sentence that expresses the fact. The *proposition* is the class of all those sentences which would, loosely, be said to "express" that proposition.[15]

4. The proposition has been defined in terms of the sentence. But what is ordinarily called "propositional form," it seems to me, needs to be linked up not so much with the symbol as with the symbolized.

4.1. At this point there is an important confusion which I

[15] A more complete analysis of proposition requires the concept of "meaning." This concept is developed in connection with "fact." And the analysis of proposition is completed in Part III.

would like to avoid. It must be noted that what is usually described as the form of the proposition is *not* structure or relation-number. For two propositions of the same form (ordinary use) may not have the same structure in the technical sense of isomorphism. I believe that what most modern logicians mean by propositional form is cardinal number. Take these two propositions "A loves B" and "C precedes D." In most treatments the two propositions are said to have the same form, that form of which x R y is supposed to be an adequate map. But they have not the same relation number, for if they did, their components[16] would have the same formal properties. And this is not the case. "Loves" is non-transitive, non-symmetrical, and non-reflexive. "Precedes" is transitive, asymmetrical and irreflexive.

It seems that all that is necessary for two atomic propositions to have the same form is that there be a one-to-one correspondence between their constituents. The view of many contemporary logicians is that "every proposition is composed of one or more substantive elements and an attributive or relational element."[17] Thus if a proposition has two constituents it is a subject-predicate proposition; if it has three, it is a dyadic relational proposition and so on.[18] The number of elements is equal to the number of substantives plus one—the relational or attributive element. And the number of major elements *is* the form of a proposition, on this view.

So *Russell Int. Mat. Phil. page 99:* "the form of a proposition is that, in it, that remains unchanged when every constituent of the proposition is replaced by another."[19] *ibid. page 201:* "A fundamental logical constant will be that which is in common among a number of propositions, any one of which can result from any other by substitution of terms one for another."

[16] If a R b, R is the component.

[17] *Eaton:* Gen. Logic, page 58. Eaton says "every proposition." As the title of this section makes clear, I offer here an analysis only of "atomic propositional form." Most of the logicians cited have not been clear on this point. Witness the passages quoted.

[18] *Eaton:* Gen. Logic, page 58, and *Russell: Knowledge of Ext. World*, page 54. For a fuller presentation of this view see the works mentioned. Also *Stebbing:* Modern Introduction to Logic, page 126.

[19] I don't know why Russell does not employ his principle of abstraction here. The definition of propositional form as a class is clearly possible.

Knowledge of Ext. World page 55: "two facts involving the same number of things have the same form, and two which involve different number of things have different forms."

So, too, *Eaton in Symbolism and Truth:* "Differences in logical structure are numerical in nature; they are differences in the number of constituents in groups and in the number of groups within a whole. Form is in a literal sense number, as the Pythagoreans believed."

Propositional form is, then, the cardinal number of the constituents of a proposition. And it must be repeated that this is a very different thing from relation-number. If Russell's treatment of propositional form is adequate the title of this paper may need revision. If there is *a* structure (relation-number) which the fact and the proposition have in common, it is not *the* form of the proposition (cardinal number).

I am not sure that the notion of logical form as cardinal number is adequate. As has been pointed out, it tells us little about the formal properties of the proposition, whereas relation-number tells us much. And these logical properties are what interests the logicians. Of course, there are a number of difficulties in the way of treating the form as the relation-number of the relational element of the proposition. There are, for one thing, subject-predicate propositions. In these there does not seem to be a relational element. However they might be instances, of the assertion of a dyadic relation, whose maximal generalization would be xR K or xR Q, where the relata are of a different logical type from the referents. But this is all very tentative, and the possibility of defining propositional form as relation-number is not vital for our main purpose. The important thing is to notice this ambiguity in the ordinary use of the term "form." The two alternative interpretations—as relation-number and as cardinal number—have been pointed out earlier (I—3 and 4).

4.2. I believe that what is meant by the form of a proposition is not the arrangement of the parts of a spook, but literally the form of the fact represented. This must not be taken as an answer to our main problem. That problem will be formulated later, in section 5 of this part. This is no find but a definition.

What I mean to say is: Logicians use "propositional form" and "factual form" synonymously even though they think that they represent two different concepts. I am not at all certain about so difficult a matter but it seems to me that such a treatment of propositional form has advantages and only seems paradoxical. I say this because many less paradoxical treatments boil down to the same thing.

4.3. As we have seen, the form of a proposition is treated usually as the cardinal number of the constituents of a proposition. Now what are the constituents of a proposition? They *are* the constituents of the fact, according to Mr. Mace, Miss Stebbing and Earl Russell, among others. I quote:

Stebbing "Mod. Introd. to Logic" page 161 "It is important to remember that the constituents of a proposition are not *words*, but in the case of a true proposition, are the constituents of the corresponding fact." *Page 155:* "In the case of every true proposition, the proposition has no constituent which is not a constituent of the corresponding fact."

Russell: Introduction to Mathematical Philosophy page 198: "Take (say) the proposition "Socrates was before Aristotle." Here it seems obvious that we have a relation between two terms, and that the constituents of the proposition (as well as of the corresponding fact) are simply the two terms and the relation."

Mace: Principles of Logic page 39: "Nevertheless, it is reasonable to suppose that the ultimate constituents of a proposition are also the ultimate constituents of a fact."

Now it seems to me that if the constituents of a proposition are the constituents of the fact, and if the form of a proposition is the number of the constituents of the proposition, "the form of the proposition" is another name for the form of the fact. Indeed I fail to see any distinction between the proposition and the fact on this view. The proposition is a dummy.

If this definition of propositional form is accepted, it can be seen that logic is distinct from grammar. And that what logic is interested in would seem to be the form of the fact. Thus two sentences, syntactically the same, do not necessarily express facts of the same form. "Lions are hunted" and "Unicorns are

thought of" are grammatically the same, though of different propositional forms. Expressing these facts in syntactically the same sort of sentence does not change their forms. It fails to analyze them. On the view suggested the study of propositional forms becomes an empirical question, rather than a philological one. This notion of logic as treating of propositional forms a *posteriori*, has been suggested by Dr. Nagel (in lecture), Mr. Wittgenstein (in his article on "Logical Form") and Mr. Ryle.[20] The task of logic, in this connection, would not be an examination of the arrangement of marks on paper. Logic would analyze the forms of facts and perhaps try to express these facts in sentences exhibiting the same forms. So the Principia Mathematica and the Theory of Descriptions.

5. I think we are now in position to formulate the problem. The proposition, as we have seen, is a class of sentences, or to bring this paper right up to date, a logical construction out of sentences. It is clear that the proposition so defined is not the sort of entity that can be relasimilar to the fact. If this view is accepted we are not left without a problem, however. The problem is merely fixed. For there is the sentence—the sinsign—which may or may not be isomorphic with the fact. And some of the philosophers who advance the thesis we are examining have stated explicitly that they mean the sentence "in all its particularity."

Now I do not wish to deny that it is possible to define the proposition and its constituents in such a way as to make isomorphism possible. Indeed I believe that they may be so defined as to make isomorphism inevitable. And I shall consider this later. Because of the ambiguity in the use of the word "form" and in the statement of what it is that has the same structure as the fact, we can now distinguish four of the different interpretations of the thesis under examination.

(a) The proposition is classimilar to the fact. That is to say, the proposition has the same cardinal number of constituents as the fact. The truth of this depends upon the definition of

[20] Op. cit., p. 126.

propositional constituents. It does not assert any isomorphism properly so called.

(b) The sentence (sinsign) is classimilar to the fact. This is the view stated explicitly by John Wisdom. It is also, possibly, the view of Wittgenstein and Eaton. This view, likewise, does not assert any isomorphism properly so called.

(c) The proposition is relasimilar to the fact, has the same relation-number.

(d) The sentence is relasimilar to the fact. This is the possibility Leibniz seems to have considered. Here "isomorphism" is used properly. If this view is sound, the sentence is isomorphic with the fact in precisely the same sense as the map.

Consistency and Independence in Postulational Technique

BY

ARNOLD F. EMCH

INTRODUCTION

DESPITE the skepticism of many mathematicians and logicians as to the possibility of any test which will show conclusively the consistency or independence of the members of a postulate set, several methods have nevertheless been devised and employed,[1] e.g., the empirical methods of Russell and Huntington, the internal method of Hilbert, and the reflective method of Royce. However, with the possible exception of Hilbert's method (which is still available only in the form of fragments and suggestions), these techniques require us to forsake the purely formal or abstract mode of analysis,[2] and instead to rely in some sense on "concrete representations" or "interpretations" or "modes of action" which are said to "satisfy" the postulates in question. In short, they are all interpretational methods—and here is the crux of the problem. "Is it possible," queries Weiss, "that the only way we can determine whether a

[1] For an excellent discussion and classification of various tests for consistency, cf. H. M. Sheffer's doctoral dissertation in the Harvard College Library, specifically the chapter on *Consistency of Propositions.*

[2] According to Lewis, Sheffer has, in a privately circulated manuscript, *General Theory of Notational Relativity*, "offered a general method for testing consistency and independence without reference to any possible application." Lewis adds that "the method of dealing with usual branches in this way must wait upon his further publication." C. I. Lewis, *Mind and the World Order*, New York, 1929, p. 244n.

set is consistent is by seeing all the postulates actually exemplified in some object? If so, we must arbitrarily assume that the object is self-consistent, so that the proof of consistency must ultimately rest on a dogma. As independence rests on consistency there are therefore no satisfactory proofs as yet of either independence or consistency."[3] The question "is thus answered," according to Young, "only by reference to a concrete representation of the abstract ideas involved, and it is such concrete representations that we wished especially to avoid. At the present time, however, no absolute test for consistency is known."[4] Moreover, "suppose," says the late Dr. Eaton, "that the system has no interpretation: how can the consistency (and) independence . . . of the postulates be shown? There should be some analytic way—purely in the realm of the abstract, without interpretation—of establishing these properties of a set of postulates. This is an important problem that awaits solution."[5]

The present study is intended to indicate some of the logical problems that arise in the consideration of an analytic test for consistency and independence and to suggest certain possible solutions of these. The study includes, in §1, an analysis of the primitive set-up for abstract deductive systems in order to ascertain precisely what constitutes a "postulate," in §2, an analysis of the meaning of "consistency" and "independence" in accordance with the methodological requirement that members of a postulate set be both consistent with and independent of each other, and in §3, a resolution of the problem of consistency and independence into the problem of non-tautology.

§1. MEANING OF POSTULATE

The solution of the problem of consistency and independence in connection with the members of a postulate set requires, first, an analysis of the primitive set-up for abstract deductive systems, with the aim of ascertaining the general nature of the various

[3] Paul Weiss, "The Nature of Systems," *The Monist*, vol. xxxix (1929), p. 468.

[4] J. W. Young, *Lectures on Fundamental Concepts of Algebra and Geometry*, New York, 1930, p. 144.

[5] R. M. Eaton, *General Logic*, New York, 1931, p. 474n.

expressions which are required to be consistent with and independent of each other. It has been argued, e.g., by Huntington, that the premises of any mathematical system, in the abstract, "are to be regarded no longer as 'axioms,' since they are merely blank forms, not in themselves either true or false, but rather as 'postulates,' because we 'demand,' arbitrarily, that the system should conform to these conditions."[6] These postulates, he adds in a subsequent article, are strictly speaking "not propositions at all, but rather what Bertrand Russell[7] has called 'propositional functions,' which become propositions (true or false) only after particular values are assigned to the variable symbols" which constitute the base for the set.[8] Sheffer corroborates this point of view when he says that "inasmuch as the base for a postulate set for a given division of mathematics consists, not of classes and relations, but of class functions and relational functions (class functions are to classes, and relational functions are to relations as propositional functions are to propositions) the postulates consist, not of propositions but of propositional functions."[9] The question now arises whether the point of view, as expressed in these quotations, is a correct interpretation of the case. The usual procedure in the codification of a purely mathematical or logical system is to assume (1) a class K, (2) one or more relational (or operational) variables, R, etc., and (3) the properties of K, R, etc.,—which constitute the postulates of the set. But what does it mean to assume (say) a class K *and* a relation or rule of combination R *and* a set of properties *of* K and R? Whatever else it may or may not mean, it does suggest that any postulate of the set is such that "for some choice of K, such and such a

[6] E. V. Huntington, "The Fundamental Laws of Addition and Multiplication in Elementary Algebra," *Annals of Math.*, 2nd series, vol. viii (1906), 1.

[7] *Principles of Mathematics*, Camb. Univ. Press, 1903, or L. Couturat, *Principes des Mathematiques*, Paris, 1905.

[8] E. V. Huntington, "The Fundamental Propositions of Algebra," in J. W. A. Young, *Monographs on Topics of Modern Mathematics*, New York, 1932, pp. 164–172. Cf. also, G. Frege, "Über die Grundlagen der Geometrie" in *Jahresbericht der deutschen Mathematiker-Vereinigung*, vols. xii (1903), xv (1906), and A. N. Whitehead, *The Axioms of Projective Geometry*, Cambridge, 1913, chap. 1.

[9] H. M. Sheffer, "Review of 'Principia Mathematica,'" *Isis*, vol. viii (1926), pp. 226–231.

property of K holds" or "for some choice of K and for some choice of R, such and such a property of K and R holds." Since "choice" here refers to the alternatives covered by the range of significance of the variables K and R, these variables appear to be at least implicitly generalized in the postulates in which they occur. Since, further, any "elements" of the class K are always (if not explicitly) generalized in the postulates, there remain, strictly speaking, no "free"[10] variables in any postulate of the set. But any expression in which all the variables are generalized is not a propositional function but a general or a purely *logical proposition.* Hence if postulates are such that "for some choice of K, R, etc., such and such a property of K, R, etc., holds," then postulates—contrary to the accepted belief—are not propositional functions at all but propositions indeed capable of being true or false. Although some difficulties beset this point of view, there is further evidence to substantiate the theory that postulates are at least implicitly general or purely logical propositions. Any set of postulates is conceived to imply some set of theorems, which is equivalent to saying that if the members of a set of postulates are true, then the members of some set of theorems will be true. But just as it is a characteristic of propositions that they are capable of being true or false, so it is characteristic of propositional functions that they are incapable of being true or false. Hence if postulates should be conceived to be propositional functions, then we would be confronted with the perplexing situation of a set of premises the members of which are incapable of being either true or false, but which nevertheless, if true, imply the truth of such and such a set of theorems. But what can it mean to say that if the members of such and such a set of premises are true, then the members of such and such a set of theorems will be true, when, in point of fact, the assertion that one or more of the premises is true, is strictly speaking, non-sense? This is plainly a contradiction in terms. Any attempt to escape this criticism by maintaining that a postulate *is* capable of being true or false—but only in the sense that it

[10] A variable is said to be "free" when it is neither generalized nor specified by the assignment of a value to it.

becomes a proposition (either true or false) when values are assigned to the undetermined constituents which it contains, would be a palpable evasion of the issue, viz., whether any theorem which is conceived to be incapable of being true or false is *deducible* from some set of premises whose members are likewise conceived to be incapable of being true or false. The contention here is that even though a propositional function may imply some other propositional function in the sense that the implication between them will hold for all values of the variables in the functions, the first function, if it is to serve as a premise, must be assumed, which fact involves the presumption that it is at least assertable, possessing a completeness and unity of meaning such that it has a capacity for truth or falsehood—otherwise no theorem which is implied could be expressed unconditionally, e.g., in *Principia Mathematica*. Thus a propositional function can serve unambiguously as a premise only if its variables are either generalized (at least implicity) or specified by the assignment of values to them, in which case it is no longer a propositional function but a proposition. Since mathematical systems in the abstract are in fact developed deductively without appeal to interpretations, i.e., without the assignment of values to the variables constituting the base for the postulate set, it follows that either there is an ambiguity in the concept "deducibility" or else the variables in the postulates must be at least implicitly generalized such that each postulate is genuinely assertable. The importance of this to postulational technique is that if consistency and independence are definable in terms of "deducibility," then consistency and independence could not be significantly predicated of a set of postulates, if "postulate" were taken to mean "propositional function."

§2. MEANING OF CONSISTENCY AND INDEPENDENCE[11]

Now if, according to the ordinary procedure in postulational technique, the members of a postulate set are required to be both consistent with and independent of each other, then at least it

[11] I am indebted to Dr. William T. Parry of Harvard University for certain criticisms and suggestions in connection with this section.

should be the case that their consistency is not inconsistent with their independence. The significance of this statement lies in the fact that it determines the meaning which is to be attached to consistency and independence. If, for example, the consistency of two propositions, p and q, is taken to mean that "p does not materially imply[12] the falsity of q," and q's independence of p that "p does not materially imply q," then it should be the case that " 'q's independence of p' is consistent with 'the consistency of p and q.' " But in *Principia Mathematica*, Whitehead and Russell prove a theorem which is contrary to the required principle. Hence, if consistency and independence are defined in terms of material implication, q's independence of p is inconsistent with the consistency of p and q, which is materially equivalent to saying that no proposition can be at once consistent with and independent of another.

In contrast to the failure of material implication to allow propositions which are both consistent with and independent of each other, it is one of the merits of the calculus of strict implication,[13] as developed by Lewis, that it is at least partially in accord with the requirements of postulational technique. Since these requirements are that (1) p is consistent with q, and (2) q is independent of p, and (3) p is independent of q, the complete function which must be shown to hold in any calculus which is designed to express the facts about the relations of propositions commonly denoted by "consistency" and "independence" is that for some propositions, p and q, the conjunction of (1), (2), and (3) holds.

The calculus of strict implication does not contain this principle, but instead the existence postulate (20.01) which states that "there is some pair of propositions, p and q, so related that p implies nothing about the truth or falsity of q."[14] This is logically equivalent to the statement that "there are some propositions, p and q, such that q is both independent of and consistent

[12] "p materially implies q" means "it is not the case that p is true and q false."

[13] "p strictly implies q" means "it is not the case that it is possible that p should be true and q false."

[14] C. I. Lewis and C. H. Langford, *Symbolic Logic*, New York, 1932, p. 179. In symbols $(\exists p, q):\sim(p \prec q)\cdot\sim(p \prec \sim q)$.

with p." The question of including this or the former principle in a system of logic is of course purely pragmatic. If Lewis is interested only in categorically distinguishing his system from that of material implication, then his existence postulate will indeed serve his purpose; but if he is interested also in devising a logic which will satisfy the requirements of consistency and independence in postulational technique, then he should postulate the needed principle, i.e., "for some propositions, p and q, p and q are both consistent and independent."

Now since the function in this principle has only two variables, p and q, the only kind of postulate set for which it could hold (in the ordinary sense) is a set of two. Hence, from the point of view of generality, the function should be extended to include any number of variables such that some propositions, p, q, r, s, . . . , are both consistent and independent. It is important to note that while the statement "p, q, r, s, . . . are all consistent" or "p, q, r, s, . . . form a consistent set" means "the joint-assertion of any selection from p, q, r, s, . . . is consistent with the joint-assertion of the remainder of the set," the statement "p, q, r, s, . . . are all independent" or "p, q, r, s, . . . form an independent set" does *not* mean "the joint-assertion of any selection from p, q, r, s, . . . is independent of the joint-assertion of the remainder of the set." It *does* mean "the assertion of any one member selected from p, q, r, s, . . . is independent of the joint-assertion of the remainder of the set."

The problem now arises whether (and if so, how) it can be shown for some propositions, p, q, r, s, . . . that our general requirement holds. The tests usually employed by mathematicians and logicians for the consistency and independence of a given set of postulates, based as they are on the supposition that postulates, as previously indicated, are propositional functions, constitute a transformation of all the functions of the set into propositions. If, on the one hand, the propositions resulting from the assignment of values to the variables in the functions are all true, then we are said to have an interpretation satisfying the functions or a true interpretation—in which case the postulates are conceived to be consistent. Such a test is based, at least implicitly, on the

principle that if "p, q, r, s, . . . are all true propositions, then p, q, r, s, . . . are consistent," and further, on the supposition that if p, q, r, s, . . . are consistent, then the corresponding functions are consistent. If, on the other hand, the propositions resulting from the assignment of values are all true except one, then the postulate corresponding to the proposition that is false is conceived to be independent of the remainder of the set. Such a test is based on the principle that "if q, r, s, . . . are all true and p is false, then p is independent of qrs . . . ," and further, on the supposition that if p is independent of qrs . . . , then the corresponding function must be independent of the remainder of the set. When, by successive interpretations, the respective sets of propositions are such that qrs . . . $\sim$ p, prs . . . $\sim$ q, pqs . . . $\sim$ r, pqr . . . $\sim$ s, etc., then all the postulates of the set are conceived to be independent of one another. Hence, in order to establish the independence of a given set, it is required to find as many interpretations as there are postulates in the set. But many logicians and mathematicians regard these tests as theoretically unsatisfactory, and there are not a few who seem to repudiate the principles upon which they are based. The crux of the problem seems to be (a) that consistency and independence appear thereby to be established only by reference to empirical fact—which is precisely the sort of reference, e.g., in geometry, that such tests should avoid, (b) that the absence of relevant empirical data does not preclude the possibility that the propositions of any given set are consistent or independent, and (c) that such tests involve complications, e.g., the significance of consistency and independence as applied to any set of propositional functions.

Whatever the merits of the so-called empirical method, a solution to the problem of an analytic test is suggested in the principle "if p, q, r, s, . . . are all necessary propositions (or necessarily true), then p, q, r, s, . . . are consistent." But what is a legitimate test which, when satisfied, enables us to recognize a necessary truth? It will be recalled that traditionally such a test is found in the reflective method or "reaffirmation through denial," i.e., if the denial of a proposition p requires the affirmation of p,

then p is necessarily true.[15] However, even though this method has been glorified as "a veritable foundation stone for the rationalistic procedure,"[16] it is nevertheless open to criticism at least on the grounds (a) that it is ambiguous, (b) that it condones vicious circle fallacies, and (c) that it is really not a test of truth at all. Lewis has aptly said that "any case in which this test is supposedly used to establish truth should be subject to close scrutiny; there is always a colored gentleman in the woodpile."[17]

Colored gentlemen notwithstanding, there is a class of necessary truths such that each is implied by its own denial; but this characteristic is not a test of logical truth, but merely an explication of the structure that is implicit in any truth of this kind. Now if "p is implied by its own denial" or "the denial of p is not self-consistent" is formally or logically equivalent to "p is necessary," then, according to Lewis, "necessary truths, so defined, coincide with the class of tautologies, or truths which can be certified by logic alone."[18] Hence all that need to be shown for the consistency of p, q, r, s, . . . is that p, q, r, s, . . . are all tautological. And the test which, when satisfied, enables us to recognize a tautology, is the matrix test of any truth-value principle or law. Since by this method tautologies are certifiable by logical analysis alone, interpretation in the ordinary sense not being required, it is possible to determine consistency by purely analytic procedure and without reference to empirical fact.

This test, however, fails to be universally applicable. First, it is not the case that all necessary truths can be certified by the matrix method, since there are other necessary truths than those which are determined by the truth-values of their elements; and second, it is a logical correlate of necessary truths, as here defined, that "if p, q, r, s, . . . are all necessary, then p, q, r, s, . . . are all

[15] J. Royce, "Principles of Logic," in A. Ruge, *Encyclopaedia of the Philosophical Sciences*, London, 1913, vol. i, pp. 120–125.

[16] G. Cardanus referred to it as "the most wonderful thing that has been devised since the creation of the world." Cf. "The Logica Demonstrativa of Girolamo Saccheri," by the present author, in *Scripta Mathematica*, vol. iii (1935), nos. 2 and 3.

[17] C. I. Lewis, "The Structure of Logic and its Relation to Other Systems," *The Journal of Philosophy*, vol. xviii (1921), no. 19, pp. 507–510.

[18] *Symbolic Logic*, *op. cit.*, pp. 209–211, 284.

logically equivalent."[19] The significance of this principle for postulational technique is that if the members of a set of postulates are all necessary, then they are not independent. Since all logical propositions, and, according to Lewis, all the laws of pure mathematics, are tautological and hence necessary[20] it follows that either (a) no members of any set of logical propositions or principles of pure mathematics are independent, or (b) the definitions of consistency and independence offered in the calculus of strict implication do not represent correct analyses of these concepts as employed in postulational technique. It should be apparent that the first of these alternatives is contrary to fact, for if no members of any set of logical or mathematical propositions were independent, then the whole of any mathematical system or system of logic could be derived from a single postulate of the set. This is not, of course, the case.

The point in this connection is that the properties of strict implication do not in fact coincide with those of deducibility as employed in the deductive development of logic or mathematics. Any one of the paradoxical theorems of strict implication, e.g., "a proposition which is impossible implies any proposition" or "a proposition which is necessarily true is implied by any proposition," is a case in point. Moreover, if we should take "p is consistent with q" to mean "p does not strictly imply the falsity of q," and "q is independent of p" to mean "p does not strictly imply q," then in terms of strict implication no two logical or mathematical propositions could be at once consistent and independent.

It will be required, therefore, to find an implicative relation which will be in accord with the meaning of deducibility in the deductive development of logic and mathematics and which, further, will allow at least some of the propositions of logic and mathematics to be both consistent with and independent of one another. Such a relation, at once narrower and stronger than strict implication, can be defined in terms of negation, possibility and product:

$$p \backsim q. = . \sim \Diamond \Diamond (p \sim q),$$

[19] *Ibid.*, p. 176.
[20] *Ibid.*, pp. 24, 211–212.

and can be read "p logically implies q," which is defined to mean "it is impossible that p true and q false is possible." The detailed properties of this relation need not be developed here; it is sufficient to note that an entire calculus of propositions based upon this meaning of "implies" can be developed such that (a) it will accord with the meaning of "deducibility" as employed in the deductive development of logic and mathematics, (b) the paradoxical theorems of strict and material implication are obviated, (c) logical and mathematical propositions can be both consistent and independent, and (d) the entire calculus of strict and hence material implication can be deduced from it.[21]

§3. NON-TAUTOLOGY

It can be shown, further, that when the implicative relation, $p \backsim q$, holds, it always expresses a tautology such that q is logically deducible from p. Hence the presence of non-tautology indicates non-deducibility in this sense. Now if consistency and independence are defined in terms of a denial or negation of this implicative relation, then all that need be shown for the consistency of a pair of propositions, p and q, is that the implicative relation between p and $\sim q$ is such that it is not tautological and for q's independence of p that the implicative relation between p and q is not tautological. If "tautology" is taken to mean "an exhaustive set of possible alternatives," non-tautology simply means that the alternatives are non-exhaustive or incomplete. Thus the problem of consistency and independence is reduced to the problem of non-tautological or non-exhaustive sets. Since it is characteristic of tautologies that they are certifiable by logical analysis alone, interpretation in the ordinary sense not being required, it would seem to be possible to determine consistency and independence by purely analytic procedure and without regard to the usual interpretations. It would be required merely to determine by logical analysis whether or not the set of proposi-

[21] For further discussion with respect to the inadequacy of strict implication as a relation to be used for purposes of inference, the significance of "$p \backsim q$" in the theory of deduction, and the calculus based upon it, cf. the present author's paper, entitled "Implication and Deducibility," to be published in *The Journal of Symbolic Logic.*

tions in question constitute a tautology—and this would be done without reference to the universe or the facts of nature.

The one serious doubt as to the practicability of non-tautology as a test of consistency and independence would seem to be in the lack of any finitely applicable formulae for non-tautology. It appears that non-tautology can be indicated only indirectly by reducing a set of propositions to such terms which, if tautological, would be recognized as such. But Lewis maintains that even though "this is doubtless a fact . . . it is merely intuition nevertheless." There is no "*rule*, applicable to all such cases (and not requiring judgments or procedures for which a rule was *not* given) the application of which would prove non-tautology."[22] Thus even the argument for the exhibition of non-tautology as an abstract analytic test for consistency and independence is rendered peculiarly ineffective by the lack of a necessary formula which, when satisfied, would enable us to recognize a non-tautology. Hence it appears that if an analytic test for consistency is to be found (which test will not preclude the possibility of independence), it will be required to devise a finitely applicable formula for non-tautology (which formulation Lewis maintains is impossible) or to treat consistency and independence in other terms than here indicated.

[22] In correspondence.

A Detail in Kronecker's Program

BY

E. T. BELL

THE PROGRAM

IT WAS Kronecker who sought to avoid the use in mathematics of all numbers (negatives, fractions, irrationals) other than the positive integers, and he outlined the means for carrying through this program.[1] In the introductory sections of his memoir he briefly indicates the personal philosophy which made such a project appear desirable.

It does not seem surprising that Kronecker's program has inspired controversies and created its own opposition. The program itself has been called bizarre, impracticable, and idle even if it were possible of achievement. The general sentiment on the hostile side has been briefly summed up as follows by Hobson.[2]

"His [Kronecker's] ideal is that every theorem in analysis shall be stated as a relation between integral numbers only, the terminology involved in the use of negative, fractional, and irrational numbers, being entirely removed. This ideal, if it were possible to attain it, would amount to a reversal of the actual historical course which the science has pursued; for all actual progress has depended upon successive generalizations of the notion of number, although these generalizations are now regarded as ultimately dependent on the whole number for their

[1] L. Kronecker, Uber den Zahlbegriff, *Journal für die reine und angewandte Mathematik* vol. 101 (1887) pp. 251–280; Werke, vol. III 1, pp. 251–280.

[2] E. W. Hobson, *The Theory of Functions of a Real Variable*, etc., (1907) Art. 17.

foundation. The abandonment of the inestimable advantages of the formal use in Analysis of the extensions of the notion of number could only be characterized as a species of Mathematical Nihilism."

The doubt whether it is possible to attain Kronecker's ideal has recently been repeated.[3,4] It must be remembered, however, that Kronecker wrote his sketch at a time when it was not customary, as it is now, to state carefully the postulates upon which a particular mathematical theory is based before proceeding to the construction of the theory. Further, parts of his exposition are quite concise and might easily be misinterpreted by a reader as antipathetic to Kronecker's program as Couturat was. Couturat let himself go; he attacked the program with gusto and damned it in general and in detail. Some of his objections are easily seen to be based on a simple misunderstanding of parts of Kronecker's elementary algebra. This applies in particular to some of the strictures on Kronecker's theory of negatives, and a similar oversight occurs in connection with objections to the treatment of irrationals and fractions.

Although it may be presumptious to attempt to convince anyone temperamentally opposed to Kronecker's program that it is not the bizarre futility it is sometimes said to be, nevertheless it may be recalled that some of the techniques outlined or used by Kronecker in his memoir and in its companion on algebraic "magnitudes" have had, and still have, wide application in theory of algebraic numbers, the theory of algebraic functions and the theory of modular systems, all of which are severely technical mathematical disciplines untainted by speculative impracticality or mathematical nihilism. In short, Kronecker's mathematics in his sketch of the program, *as mathematics*, needs no defense. Nor is it necessarily true that the historical development of the number system is the best for a modern presentation. The historical development is, in fact, not followed: we

[3] A. C. Black, *The Nature of Mathematics* (1933), p. 177. Black refers "for a full discussion and criticism of Kronecker's views" to

[4] L. Couturat, *De l'Infini Mathématique*, (1896), pp. 603–616. This book had a wide circulation a generation ago.

do not torture ourselves as the ancients vainly did to produce a civilized theory of common fractions; we do not repeat, even in the elementary schools, the endless arguments which the early algebraists found necessary before they would admit negative numbers to a parity with the positives; nor do we trouble our souls with profound doubts about "sophistical quantities" when some equation incontinently presents us with an imaginary root. To attack Kronecker's program on the ground that it is unhistorical is to render aid and comfort to the enemy.

Before going on to the simple detail in connection with negative numbers, we recall that Cauchy[5] originated the device of algebraic congruences as an alternative, "real," way of handling complex numbers. This device is that which Kronecker extends and exploits to the limit to give him the algoristic half of his theory. The other, less tangible, half concerns Kronecker's theory of adjunction and is much harder to justify to a hostile critic of the program and to clear once and for all of the charge of circularity. The historical origin of the abstract principles behind the theory of adjunction can be traced, I believe, to Kummer, who was one of Kronecker's teachers. Kronecker's specific application of the principles is entirely his own.

NEGATIVE NUMBERS

The theory is based on the concept of what is now called a *semi-field*. It will be sufficient to define the terms used, state the postulates, and give only enough of the theorems deduced from the postulates to dispose of one objection which has been raised against Kronecker's algorithms. The proofs of the theorems are almost immediate from the definitions and postulates. What follows is an abstract of the first part of a paper by Wedderburn.[6] The postulates for equality, $=$, are presupposed.

[5] A. L. Cauchy, *Exercises d'Analyse et de Physique Mathématique*, Tome 4, (1847), pp. 87–110. One remark of Cauchy's is rather amusing, coming as it does from the inventor of the theory of functions of a complex variable:

"In the theory of algebraic equivalences [congruences] substituted for the theory of imaginaries, the letter i will cease to represent the symbolic sign $\sqrt{-1}$, which we repudiate completely, and which we can abandon without regret, because nobody knows what this sham sign signifies, nor what meaning is to be attributed to it."

[6] J. H. M. Wedderburn, Algebraic Fields, *Annals of Mathematics, Second Series*, vol. 24 (1923), pp. 236–264.

Let D denote a set of elements (or "marks") $a, b, c, \ldots$, finite or infinite in number, which satisfy the following postulates.

P. *If a, b are any elements of D, not necessarily distinct, there exist two single-valued functions of a, b, denoted by $a + b$ and $a \times b$ respectively, and these functions have the following properties.*

A_1. $a + b$ *is an element of* D.
A_2. $a + b = b + a$.
A_3. $a + (b + c) = (a + b) + c$.
A_4. *If* $a + b = a + c$, *then* $b = c$.
A_5. *There is an element* 0 *such that* $0 + 0 = 0$.
M_1. $a \times b$ *is an element of* D.
M_2. $a \times b = b \times a$.
M_3. $a \times (b \times c) = (a \times b) \times c$.
M_4. *If* $a \times b = a \times c$, *but* $a + b \neq b$, *then* $b = c$.
M_5. *There is an element* $1 \neq 0$ *such that* $1 \times 1 = 1$.
AM_1. $(a + b) \times c = a \times c + b \times c$.
AM'_1. $c \times (a + b) = c \times a + c \times b$.
AM_2. *If* $a \times c + b \times d = a \times d + b \times c$, and $a \neq b$, then $c = d$.

The "0" in A_5 might be denoted by z, and the "1" in M_5 by u, to emphasize that these are not necessarily the zero and unity of common arithmetic. But as there can be no confusion, we let 0, 1 stand as they are.

A set D satisfying these postulates is called a *semi-field* (because the inverses of $+$, $\times$, which would give subtraction and division, as in a field, are not postulated). Note that the elements $a, b, c, \ldots$ of D are *any* marks satisfying the postulates. The point of departure is thus set farther back than Kronecker's, in which the positive integers are taken as the starting point. This (as Wedderburn remarks) is merely a matter of convenience for what is to follow. It is evident that an *instance* of a semi-field is obtained from D when we take as the elements the positive integers and zero, and for $+$, $\times$ the plus, times of common arithmetic. To prevent a possible misunderstanding, the $+$, $\times$ of the postulates might be enclosed in circles, as is sometimes done, but this is unnecessary.

The argument will be clearer if we state first what is to be done, following Cauchy and therefore also Kronecker. What immediately follows is merely descriptive. Assuming, known say, the real numbers, we can *classify* all the polynomials with real coefficients in the *real variable* i by means of the "division transformation" with respect to a given polynomial, say $i^2 + 1$, as follows. Dividing the polynomial $f(i)$ by $i^2 + 1$ we get a quotient $q(i)$ and a remainder $r(i)$, both of which are polynomials with real coefficients, and the degree of $r(i)$ is less than that of the divisor. Thus, when the divisor is $i^2 + 1$, we can express $f(i)$ in the form

$$f(i) = q(i)\,(i^2 + 1) + ai + b,$$

where a, b are real. Different polynomials may give the same remainder. All those polynomials which give the same remainder $ai + b$ are defined to belong in the same *class*. A particular class contains one and only one remainder, say $ai + b$. Hence this class, being completely specified by the unique remainder $ai + b$, may be denoted by $\{ai + b\}$, and $ai + b$ may be taken as the *representative* of the class. Any two polynomials belonging to a given class are said to be *congruent* with respect to the *divisor* as a *modulus*, or *congruent* mod the *divisor*. Here, two polynomials $f(i)$, $g(i)$ in the class $\{ai + b\}$ are congruent mod $i^2 + 1$, and this is written

$$f(i) \equiv g(i) \bmod i^2 + 1,$$

which is merely an expression of the fact that $f(i)$ and $g(i)$, when divided by $i^2 + 1$ give the same remainder $ai + b$. We need not go into the excellent reasons for adopting this seemingly involved manner of stating an elementary fact; it is sufficient to emphasize here that the *remainders* $ai + b$ and the corresponding *classes* $\{ai + b\}$ are the important things for our present purpose. If the divisor is any polynomial with real coefficients, the procedure is the same.

Suppose, however, that we are not operating with polynomials whose coefficients are any *real* numbers (positive, negative, fractional), but have restricted ourselves to polynomials whose

coefficients are elements of the set 0, 1, 2, 3, ... of common arithmetic. Then it is easily seen that, *in the form of the division transformation furnished by common division, as in elementary algebra, the preceding classification breaks down.* For there will be *remainders* in which the coefficients are *negative* or *fractional*, and *neither the negatives nor the fractions* are included in the set 0, 1, 2, 3, ... For example, dividing $x^2 + 2x$ by $x^2 + 1$ we get

$$x^2 + 2x = 1\,(x^2 + 1) + 2x - 1;$$

the remainder is $2x - 1$, and we might be tempted to say that $x^2 + 2x$ belongs to the class $\{2x - 1\}$. But as we have now restricted ourselves to zero and the *positive* integers 1, 2, 3, ..., the symbol -1 in $2x - 1$ has no meaning, and $\{2x - 1\}$ does not denote a class, but is entirely meaningless.

We have elaborated this because it is an example of the type of objection to Kronecker's algorithms which is based upon a simple misunderstanding of his elementary algebra. This is the kind of objection with which we are concerned here. To be specific, I shall quote (in translation) exactly what Couturat says (loc. cit. p. 613) in part with regard to Kronecker's project for avoiding the negatives, -1, -2, -3, ...

"The congruence $7 + 9x \equiv 3 + 5x \bmod x + 1$ states that the remainders of the two members, divided separately by the modulus, are equal. Now if we divide each of the binomials $(7 + 9x)$ and $(3 + 5x)$ by $(x + 1)$, we get in each case the remainder -2, that is to say, the negative number which would result from the subtractions, impossible by hypothesis."

Here it is a question of *avoiding the negatives*, and Couturat is pointing out that if we go through the elementary algebra of dividing as demanded by the congruence, then we get the negative -2, which is forbidden, negatives being, by hypothesis, meaningless when the set of permissible coefficients is restricted to be 0, 1, 2, 3, ... He gives further examples to emphasize his point. If Kronecker's algorithm did anything of this sort, the objection would be well taken. But the algorithm was carefully constructed so as to avoid this and similar pitfalls. It is clear that if we postulate only a semi-field as the domain of the coeffi-

cients, neither subtraction nor division of coefficients as in common algebra is permitted, as subtraction and division are undefined for a semi-field. Hence it must be shown that the division transformation can be constructed in a *semi-field.* Continuing with Wedderburn's paper, we abstract enough of his theorems to reach the correct form of the division transformation *for a semi-field.*

A polynomial $f(x)$ is said to be in the semi-field D if its coefficients are elements of D. *Instead of "element of D" we shall say "element"; 0, 1 are as in the postulates* A_5, M_5.

DEFINITION. Any element z such that $a + z = a$ for every a (in D) is called a *null* element.

THEOREM 1. 0 *is the only null element in D.*

THEOREM 2. $a \times 1 = a$.

DEFINITION. An element x such that $a \times u = a$ for every element of D which is not a null element is called an *identity* element.

THEOREM 3. 1 *is the only identity element in D.*

THEOREM 4. *If a is any element, then* $a \times 0 = 0$.

Let x denote any element. Then, if n denotes a positive integer, x^n is uniquely defined, by M_2, M_3.

DEFINITION. A polynomial in x is the sum of a finite number of elements and a finite number of terms of the form $a_i\, x^{n_i}$, where a_i $(i = 1, 2, \ldots)$ are given elements and x is a variable element and n_i $(i = 1, 2, \ldots)$ are positive integers.

We shall say that the polynomial is *in D.* Since $(a + b)x^m = ax^m + bx^m$, for any elements a, b, x and any positive integer m, it follows that any polynomial in x in D can be expressed in the form $a_0x^n + a_0x^{n-1} + \ldots + a^n$, where n is a positive integer and a_i are elements of D. If the degree of the polynomial is one, the polynomial is called *linear.*

The next definition is of importance in reaching the correct form of the division transformation.

DEFINITION. Two polynomials in D in the same variable x are said to be *identically equal* if, and only if, they have the same value for every value of x in D. The sign for "identically equal" is $\equiv$.

THEOREM 5. *If two linear polynomials in D are identically equal, corresponding coefficients are equal.*

That is, if $ax + b$ and $cx + d$ are in D and are identically equal, then $a = c$ and $b = d$.

THEOREM 6. *If $f(x)$ is any polynomial in x in D, then there exist two polynomials $p(x)$, $q(x)$ in x in D, and a linear polynomial $ax + b$ in D* such that

$$f(x) + p(x)\,(x + 1) \equiv ax + b + q(x)\,(x + 1).$$

It is this theorem which, in the semi-field D, replaces the "division transformation" of common algebra when the divisor is $x + 1$. The "divisor" $x + 1$ is that appropriate for Kronecker's theory of negatives; $x^2 + 1$ is the "divisor" for Cauchy's and Kronecker's theory of complex numbers, and so on; for every "divisor" which it is necessary to discuss in Kronecker's theory there is an appropriate theorem replacing the corresponding "division transformation" of common algebra, and each such theorem is the expression of an identity, corresponding to that concerning $f(x)$ in THEOREM 6. The important point to be noted in this particular case is that on each side of the sign $\equiv$ of identical equality in the theorem there is *a polynomial which is in D. We have not passed out of the semi-field D from which we started.*

DEFINITION. When, as in THEOREM 6,

$$f(x) + p(x)\,(x + 1) \equiv ax + b + q(x)\,(x + 1),$$

we say that $f(x)$ is congruent to $ax + b$ with respect to the modulus $x + 1$, and we write[7]

$$f(x) \equiv ax + b \bmod x + 1$$

The identical equality and the congruence express the same fact. Namely, if we assert the congruence, we might, if we wished, replace the congruence by the following assertion: "There exist polynomials $p(x)$ and $q(x)$ in x in D such that

$$f(x) + p(x)\,(x + 1) \equiv ax + b + q(x)\,(x + 1)."$$

[7] The sign $\equiv$ of *identical equality* in the first formula is not in danger of confusion with the same sign in the second formula, for in the latter $\equiv$ is part of the compound sign "$\equiv \ldots$ mod" of *congruence*. The "mod" indicates that congruence, not identical equality is meant.

The precise forms of the $p(x)$, $q(x)$ corresponding to a particular $f(x)$ are of no importance for our purpose.

COROLLARY (to THEOREM 6). *If the polynomials $f(x)$, $g(x)$ in x in D are such that*

$$f(x) \equiv ax + b \bmod x + 1$$

and

$$g(x) \equiv ax + b \bmod x + 1,$$

(where $ax + b$ is in D), then there exist polynomials $P(x)$, $Q(x)$, in x in D such that

$$f(x) + P(x)\,(x + 1) \equiv g(x) + Q(x)\,(x + 1).$$

The next definitions and immediate consequences of THEOREM 6 and the COROLLARY provide the basis for separating the totality of polynomials in x in D into classes.

DEFINITION. Polynomials in x in D which are congruent to *the same* linear polynomial in x in D with respect to the modulus $x + 1$ are said to be congruent to each other modulo $x + 1$.

From the definition of congruence, it follows that if

$$f(x) \equiv g(x) \bmod x + 1,$$

and

$$g(x) \equiv h(x) \bmod x + 1,$$

then

$$f(x) \equiv h(x) \bmod x + 1.$$

DEFINITION. The set of all polynomials in x in D which are congruent to the same polynomial $f(x)$ in x in D modulo $x + 1$ constitute the *residue class*, or simply *class*, denoted by $[f(x)]$, *corresponding to* $f(x)$.

DEFINITION. Two classes $[f(x)]$, $[g(x)]$ are *equal* if, and only if, any polynomial in x in D which is a member of $[f(x)]$ is also a member of $[g(x)]$, and every member of $[g(x)]$ is a member of $[f(x)]$.

By THEOREM 6, every class contains a polynomial of the form $ax + b$, which may be taken as the representative of the class.

THEOREM 7. The classes $[f(x)]$ and $[g(x)]$ are equal if, and only if, $f(x) \equiv g(x) \bmod x + 1$.

DEFINITIONS. 0,1 being as in A_5, M_5, the class [0] is called the *zero class*, and the class [1] the *identity class*. The *sum* of the classes $[f(x)]$ and $[g(x)]$ is the class $[f(x) + g(x)]$, and is denoted by[8] $[f(x)] + [g(x)]$; the *product* of $[f(x)]$ and $[g(x)]$ is the class $[f(x)g(x)]$, and is denoted by $[f(x)] \times [g(x)]$. With these definitions we have

THEOREM 8. *The set, denoted by K, of all classes of polynomials in x in D forms a semi-field.*

The *elements* in K are the classes; the $+$, $\times$ are the operations just defined in connection with clases, and the 0,1 are [0], [1]. The proof of this theorem is somewhat detailed; see Wedderburn, loc cit. pp. 247–249.

THEOREM 9. If α is any element of K there exists a unique element β of K such that $\alpha + \beta = [0]$.

We have just seen in THEOREM 8 that K is a semi-field. Hence, in particular, the elements (residue-classes mod $x + 1$) of K satisfy the postulates A_1 to A_5 for addition in a semi-field. Suppose now that we write THEOREM 9 after A_5, as is legitimate, since it has been deduced from the postulates for D, and merely re-label it A_6. Then A_1 to A_6, with $K, \alpha, \beta, \gamma, \ldots, [0], [1]$, in place of $D, a, b, c, \ldots, 0, 1$ are identical with the postulates for "addition" in a *field* (not merely a semi-field), in which subtraction, as understood in arithmetic and common algebra, is uniquely possible.[9] The element β in THEOREM 9, merely to give it a name, may be called the "negative" of α in K. Thus every element α in K has a unique negative, which may be denoted by $-\alpha$, merely as a matter of writing. We have not committed the solecism of "solving" $\alpha + \beta = [0]$ for β and getting $\beta = [0], -\alpha, = -\alpha$; all we have done is to give β a new name and a new symbol, $-\alpha$ $-\beta$, to indicate that it is the particular β prescribed by THEOREM 9 when α is given.

[8] The "+" in $[f(x)] + [g(x)]$ refers to addition of *classes*, as defined; the "+" in $[f(x) + g(x)]$ refers to addition in the semi-field D. Similarly for x.

[9] For the postulates of a field see L. E. Dickson, *Algebras and their Arithmetics*, p. 201, or the papers by Dickson or E. V. Huntington *in Transactions of the American Mathematical Society*, vol. 4 (1903) pp. 13–20, vol. 6 (1905) pp. 181–204.

It has been shown, therefore, that from any semi-field D a new set can be constructed in which "subtraction," and "negatives," having the properties of subtraction and negatives in common algebra or arithmetic, can always be constructed. The like follows for "division" and "reciprocals." Hence, from a semi-field, in which neither "subtraction" nor "division" is defined, we can construct a field. This is what was to be done.[10]

California Institute of Technology.

[10] Since this note was written, a paper by H. S. Vandiver has appeared (*Proceedings of the National Academy of Sciences*, vol. 20, November 1934, pp. 579–584) bearing partly on Kronecker's program. It would be of considerable historical interest to know the exact date of Schatunovsky's first work on objections to the unrestricted use of the law of excluded middle, as some of his examples are even more illuminating than some of Brouwer's.

Leibniz' Theory of Matter[1]

BY

J. A. IRVING

THE historic task of Leibniz was to furnish a philosophy of personality, and at the same time, and in harmony with it, a general interpretation of the physical world. He conceives therefore of a plurality of Real Beings which in their most developed form he proposes to call individuals, defining individuality in terms of unique experience. Further, he finds the monads, or so-called metaphysical points, to be centres of life, held together by their own inner or intensive force and therefore impenetrable. Consequently, when we enquire whether the Real Beings are parts of the universe, we find that since the universe is the aggregate of monadic forces, all Real Being is finite, and so in order to deal with the problem of Theism we have to reconcile the concept of divine personality with what we may define as personality in the case of human beings.

The philosophy of Leibniz resembles that of Aristotle in the sense that while an integrated system is lacking there is still the thought content necessary for such a system. Those aspects of his philosophy which are necessary for an understanding of his Theory of Matter may be briefly subsumed under three divisions: The Logical, The Mathematical, The Physical. The logical basis seemed to Leibniz to be the fundamental one. His doctrine that there are "subjects" which are never "predicates" implies a plurality of subjects. For him also, relations are internal as

[1] Read at the Thirty-fourth Annual Meeting of the American Philosophical Association (Eastern Division), New York University, December 29th, 1934.

well as external—what is a "relation" in one case is a quality in the other case, i.e. all relations are also qualities of the given. Reality is therefore a system of "relata"; the judgment is always, if complete, a relation. Therefore we could never find two objects which could be defined in such a way that their predicates are identical. Thus does Logic prescribe in advance the fate of Reality.

Leibniz' mathematical views yielded the Law of Continuity in Nature. He derived this law from two sources: (1) The theory of infinitesimals (continuity in quantity) which is yielded by the series 2, 1, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, ... (2) The theory of intensive quantity which led him to the view that no quantity is actually reducible to zero, all appearances to the contrary being cases of transformations of the extensive to the intensive. These conceptions revolutionized his treatment of the Mind-Body problem. For there are degrees of activity which can be represented on a scale which has two limits, X and −X, with zero as the threshold in between. Parallels were drawn to show the significance of this transformation of −X to X through zero; e.g. the drops of the ocean are each inaudible, but the roaring of the waves is due to the summation of the sound of each drop. There is a change of qualitative character which depends upon quantitative increment. Consequently we can have also a realm of *perception* (infinitesimal parts) which rises to a higher level—apperception. The scale is continuous; there are no gaps in nature, and it should be possible at all times to find the intermediate stages in a series.

In the sphere of the physical, Leibniz argued, as against Descartes, that matter is not primarily extension, because extension is not a "sufficient reason" for the extended. Leibniz raised the questions, "If matter is *res extensa*, what achieves the extending? What is 'distance'?" To derive distance we must have a body A so related to another body B that they do not touch. Distance depends on the plurality of reals which are 'forces.' The Cartesian problem had largely been concerned with the relation of mathematical points. Leibniz denied that mathematical points are the same as physical points. For him a

physical point is a centre of radiation of force which cannot be further contracted. The physical point "is" what it "does," or "is" where it "acts." What then is 'matter'?

Leibniz regarded matter as the appearance to a confused monad with "confused perceptions" of a group of other monads also with "confused perceptions." Since the monad has no windows it is doubtful whether any percipient monad could bear a cognitive relation to anything outside itself. I believe that Leibniz' Theory of Matter can best be understood in terms of a conceivable modification of the Causal Theory of Perception. According to Bertrand Russell's interpretation of it, the Causal Theory may be summarized as follows: "In perception we can form groups of percepts connected approximately, though not exactly, by laws which may be called laws of 'perspective.' By means of these laws, together with the changes in our other percepts which are connected with the perception of bodily movement, we can form the conception of a space in which percipients are situated, and we find that in this space all the percepts belonging to one group . . . can be ordered about a centre, which we take to be the place where the physical object in question is. . . . The essential assumption is that the group of percepts can be enlarged by the addition of other events, ranged in the same space about the same centre, and connected both with each other and with the group of percepts by laws which include the laws of perspective." (*The Analysis of Matter*, pages 216–17.)

The immediate object of knowledge in every perceptual situation is a sensibile, or group of sensibilia, which is generated by a physico-physiological series commencing in an external physical object and ending in the brain of the perceiver. Let us call sensibilia "confused perceptions" of physical objects. Physical objects are postulated to account for characteristics in the coexistence and sequence of sensibilia which seem to be not entirely haphazard in the experience of one or more observers. I do not see that there are any epistemological reasons why physical objects should not be psychical. Let us, in considering the cause of sensibilia, substitute Leibniz' pre-established harmony for the modern physico-physiological series. By doing this we get a scheme which resembles that of Leibniz.

Leibniz, like the Causal Theorists of current epistemology, considered that matter is phenomenal. But he said that it was a "phenomenon bene fundatum." According to the Causal Theory this would mean that matter is causally related to objective existence in a public world. We would expect it to reproduce some of the characteristics of its foundation, such as shape, or some characteristics, such as colour, which correspond to certain characteristics in its foundation, such as molecular structure. Leibniz says that matter is "bene fundatum" because it looks and behaves, as if it were causally related to independent existents in this way—but in fact it is not, since the monad has no windows. So that he has to show that matter possesses these very same characteristics which it would possess if it actually were causally related to its noumenal counterpart, which is a group of confused monads, defined within limits, and with the "point of view" varying continuously between adjacent members of the group.

We may consider that matter possesses five characteristics—position, extension shape and size, motion, inertia, and impenetrability. Leibniz finds a corresponding "foundation" of each of these in the group of monads. I shall now consider in order these foundations: (a) The point of view of the monad corresponds to the particular point in the piece of matter by which its position is defined. (b) The continuous extension of the piece of matter corresponds to the continuous variation of point of view among the monads. The shape and size correspond to the limits within which the point of view of different members of the group varies. (c) The phenomenon of motion corresponds to an absolute, without a relative, variation in the points of view of the monads. (d) Leibniz distinguishes between active and passive inertia. Active inertia is the tendency of moving matter to continue in movement; this is connected with what he calls the "appetition" of the monads. Passive inertia is the tendency of static matter to remain at rest until it is forced to move; this is connected with what he calls the confused appearance of the monads. (e) Impenetrability is closely connected with the experience of active inertia and is, like active inertia, referred to the appetition of the monads.

The ontological status of matter yielded by this interpretation is as follows: Matter is an element in one of the states of a monad known as a perceptual situation. I think that Leibniz would have admitted other states such as affective and conative. The problem of what constitutes "confusion" in the monad is an interesting one. On the above interpretation it might be suggested that the "confusion" does not attach to the simple awareness of the bit of matter by the monad, but to the "external referring" associated with this simple awareness of the bit of matter by the monad. In practice we act as if the bit of matter were an external and independent existent; actually the external and independent existent is the group of monads to which the bit of matter corresponds, and by which it would be caused—if the pre-established harmony had not been introduced.

It is interesting to consider why Leibniz was led to adopt this phenomenalist view of matter. His main reason would seem to be an ontological one. He argued that if matter were real, it would be a compound substance, and any given bit of it could be subdivided. But, again, it could not be a compound substance, for a compound substance consists of simple parts, and however far the subdivision of matter is carried no simple elements are reached. The conception of matter is therefore self-contradictory. What we call matter must be the confused appearance of some underlying reality of which the conception is self-consistent. Again, from the fact that any bit of matter has position, shape, and size; inertia and impenetrability; and can move, he deduces that its noumenal "foundation" must be a group of monads with a continuously varying point of view, and defined within certain limits.

In commenting on the fortunes of Leibniz' Theory of Matter in the history of thought, the following points should be noticed:

(1) The property of infinite divisibility need cast no doubt on the reality of matter. Of course it was not until long after Leibniz' time that the classic contradictions associated with the conception of infinite divisibility began to be resolved.

(2) Leibniz' distinction between "active" and "passive" inertia cannot be considered as ultimately justifiable, particularly in

view of the relational theory of space which he advocated. No absolute distinction can be drawn between rest and uniform, or even non-uniform, motion. The state which a given body exemplifies depends entirely upon the choice of axes which is made. If there were not an actual co-ordinate system relative to which a given body exemplifies any of the three states of rest, uniform or non-uniform motion, at least such a system is conceivable. This criticism would, if followed up, lead into relativist mechanics, which would be too far afield for our present purpose.

(3) It is not all certain that the "confusion" about which Leibniz speaks, is not a purely arbitrary method of justifying a pluralism which he had previously adopted. What is the standard by which degrees of "confusion" are to be estimated?

(4) Leibniz' conception of matter as living and active enjoyed a very fruitful development in the writings of the biological scientists a century after his time, fully flowering in the twentieth century when the old mechanical interpretation of the life of organisms began to be replaced by the concept of biological and physical activity.

(5) His conception that some factors are "unconscious" is in many respects the fundamental development in nineteenth century philosophy. It has certainly received a very extensive application in the writings of E. von Hartmann, Freud and Jung. The pre-established harmony also appeared in psychological thought as the doctrine of psycho-physical parallelism. And in modern psychology the theory was also advanced that molecules possess a rudimentary consciousness or power of sensation which is enhanced into consciousness when they become organized upon a definite basis. This suggestion would appear to be a revival of Leibniz' teaching that all existences are conscious, with the modern alteration that an essential corporeal nature was attributed (until quite recently) to the molecules as well.

In the light of this attempt to interpret Leibniz' Theory of Matter in terms of the Causal Theory of Perception, I shall conclude by raising a larger question: What is the justification for the pursuit of the History of Philosophy? For those who emphasize the History of Philosophy are to-day receiving the most uncompromising challenge. "What can be said at all,"

says Wittgenstein, "can be said clearly." And in the name of *absolute* clarity, Wittgenstein and his disciples have "poured out the baby with the bath" by denying that the great historical systems of philosophy can give us meaningful knowledge. But even the Logical Positivists must recognize that fossils have a value; for them the History of Philosophy becomes either a great palaeontology of the human mind or it takes on an aesthetic tone. That endless succession of philosophical systems, confidently propounded, strenuously defended like fortresses built for eternity,—must these systems become no more than sets of documents to be cherished and studied carefully for the light they throw on the intellectual evolution of our species? Must their creators frankly admit that they cannot give us meaningful knowledge—that they can merely induce us to take up emotional attitudes towards the greatest (but alas! the most "nonsensical") of all propositions, that proposition which has *The Universe* for its subject?

Above both Leibniz' Theory of Matter and the Causal Theory of Perception, hovers the fate prescribed by the Logical Positivists for most of man's discourse—the fate of meaninglessness. It is not by counter emotional attitudes that the challenge of these new positivists to the History of Philosophy is to be overcome. We shall only have taken up their challenge in a genuine fashion if we have made it evident that there has been a genuine advance in the discussion of the traditional philosophical problems during the period, say, from Leibniz to Russell. If there has been such an advance, then it is highly plausible that those great systems in terms of which man perennially interprets the austerely beautiful outline of reality are something more than myths and fancies, something more than the attempt to cure intellectual headaches. On this basis, and on this basis alone, we may confidently reject Wittgenstein's nihilistic dictum, and say with Ramsey, "we can make several things clearer, but we cannot make anything clear."

Department of Philosophy
Princeton University
Princeton, N. J.

The Philosophical Significance of the Concept of Probability in Quantum Mechanics

BY

F. S. C. NORTHROP

A STRIKING characteristic of contemporary science is its emphasis upon probability. This is especially notable in quantum mechanics. There is a respect in which probability is the same for all scientific theories. Verifiability requires that any theory predict certain numbers which can be compared with the numbers gained by actual operations of measuring. In actual practice these numbers, which we shall term theoretical measurables and operative measurables respectively, never correspond. It becomes necessary, therefore, for the scientist to specify when the deviation between them is such that verification occurs. These specifications are defined by the theory of errors in which the concept of probability has an essential place and a specific meaning. But the theory of errors is the same for all applications; hence, probability as defined by the theory of errors is the same for all scientific theories.

There is another use of probability which varies in meaning from theory to theory. This happens because it is defined in terms of the basic concepts and principles of the theory in which it occurs. It is essential that we have different names for these two uses of probability. Probability which is defined in terms of the theory of errors and which is concerned with the actual opera-

tions by means of which any theory is verified we shall call operative probability. Probability which is defined in terms of the basic concepts and principles of the specific scientific theory in which it occurs we shall call theoretical probability. Every verifiable scientific theory uses operative probability; only certain scientific theories use theoretical probability. It is with theoretical probability and theoretical measurables only that this paper is concerned.

Our question is this: What is the philosophical significance of theoretical probability in quantum mechanics?

The importance of this question is indicated by the following considerations. Because of its unique use of theoretical probability, quantum theory has been appealed to as evidence for the conclusions (1) that ontological contingency exists in nature, (2) that the principle of causality has been dismissed from science, and (3) that all propositions in science assumed to hold categorically and to express necessary connections have been replaced by more modest statements expressing merely what happens on the average or with a certain degree of probability. Certainly, these conclusions, if true, are important for philosophy. But, if so, they also merit a much more specific and technical designation of their essential connection with the fundamental concepts of quantum mechanics than has been given to date.

How is this essential connection to be determined? The meaning of theoretical probability prescribes the method to be pursued. By definition, theoretical probability depends for its meaning upon the basic concepts and principles of the specific scientific theory in which it occurs. It follows that our task is twofold: we must determine the fundamental concepts of quantum mechanics, and we must designate precisely how its particular use of theoretical probability arises out of those concepts.

THE FUNDAMENTAL CONCEPTS OF QUANTUM MECHANICS

In the following analysis of quantum mechanics I have drawn heavily upon Professors Lindsay and Margenau's excellent book *The Foundations of Physics*,[1] and in particular upon many private

[1] The Foundations of Physics, R. B. Lindsay and H. Margenau, New York, 1936.

discussions of critical points with Professor Margenau. My account differs from theirs in manner of presentation and in the number of postulates introduced, since this paper is concerned with fundamental concepts and assumptions only in so far as they are necessary to determine the precise meaning of theoretical probability in quantum theory.

Quantum mechanics in its present systematic form is a very unusual scientific theory. The best approach is (1) to appreciate the empirical and logical predicament in which physics found itself before quantum mechanics was systematically developed, and (2) to apply the method used by scientists to escape this predicament.

The predicament was as follows: Physicists knew, in the sense in which experimentally verified theory permits one to know, that physical systems behave as if they were particles. Physicists also knew, in precisely the same sense, that the same physical systems behave as if they were not particles but waves.

The method which physicists used to get out of this predicament is a very old, but theoretically-unformulated, one. When exactly described, it may be called the method of acting as if one does not know what one does know. For the benefit of those who insist upon an abstract and more dignified title we shall call it the method of simulated ignorance.

It consists in doing what its name prescribes. One proceeds as if one does not know what one does know, and to do this most effectively, one leaves the world of experimental physics and retires into one's study where one constructs the following abstract system function[2] F.

This system function F contains a number of elements s. Nothing is known concerning these elements except that they have certain unspecified properties o. Different properties o we shall designate with different small letters, q, p, m, and e. The system function F also contains certain operators O. Different operators O are designated by different capital letters. In addition to the elements s with their properties o, and the opera-

[2] I.e. a body of abstract purely logical or mathematical forms, devoid of material content; sometimes called a system form or a doctrinal function.

tors O, there are functions ϕ or ψ. Concerning the specific properties of these functions, we know as much as we act as if we know about everything else, which is theoretically nothing. This ignorance upon our part is exceedingly fortunate for it permits us to choose almost any purely formal mathematical function for ϕ or ψ. Only certain very general restrictions are prescribed. Moreover, these restrictions are almost, but not entirely, independent of the elements s in the system function F. Let us define the very general restrictions upon them formally by the postulates of another system function which we shall call F'.[3]

In point of fact, our physicist, turned mathematical logician, knows, as physicist, precisely what the definition of F' must be.

But his method of acting as if he does not know what he does know will not permit him to express this knowledge in stating the postulates for F'. Moreover, there is an exceedingly good reason why this is the case. For if he based his system function F' upon what he knows he would be rearing it upon those concepts which lead to the contradiction from which he is trying to escape.

Hence, he makes a guess at how F' is to be defined, and since there is no more reason for one guess than another, he picks one by chance and writes down the postulates for F' which his knowledge would have told him to be correct had he forgotten that he does not know what he does know. To be sure this does not seem to be quite fair. But he will assure you that it is,[4] for he could have written down a different set of postulates for F' and then gone on to complete the system function F. He would have found in the end when he returned to the laboratory that the deductions from such assumptions did not accord with observation. This would have forced him to try the method of simulated ignorance over again. Finally, by this process he would eventually arrive at the proper postulates for F'. This being the case, I am sure you are not merely satisfied but relieved that he guessed correctly the first time.

[3] The postulates of F' could have been included in those of the system function F. I have separated them in order to distinguish very general properties from more restricted and technical ones; also, for reasons of emphasis which will appear later.

[4] Ibid., p. 403.

Having designated the base of the system function F, he now proceeds to designate its postulate. Postulate I asserts that to any property o of an element or elements s of F there corresponds an Operator O. The precise correlation is found again by the method of simulated ignorance in conjunction with trial and error.[5]

The purely formal properties of O are such that an interesting consequence follows, which we shall term Theorem 1. Theorem 1 asserts that an operator O for a given property o generates a sequence of real numbers o_λ according to the equation

$$O\psi_\lambda = o_\lambda \psi_\lambda,$$

where the o_λ and the ψ_λ are what pure mathematicians term characteristic numbers and characteristic functions respectively.

The essential point in this theorem is that the operator O generates a definite set of o_λ numbers which are in correspondence with the ψ_λ functions.

One more consequence of the system function F is needed for our purposes. It can be deduced from the properties of the function ϕ by purely mathematical principles. We shall designate it as Theorem 2. This theorem asserts that the equation $\phi = \sum_\lambda a_\lambda \psi_\lambda$ holds, where a_λ is another set of numbers, also in correspondence with the ψ_λ functions.

It follows, therefore, from Theorem 1 and Theorem 2, because of the transitive property of this relation of correspondence, that the o_λ and a_λ numbers are in correspondence with each other. This consequence, we shall call Theorem 3. There is another interesting consequence of our system function F which we shall term Theorem 4. It will be mentioned later.

We will gain from these technicalities everything essential for the purpose of this paper, if we concentrate attention upon the following simple fact: The few abstract assumptions of our system function F are sufficient to generate two sets of numbers o_λ and a_λ which are in correspondence with each other. The following important properties of these two sets of numbers should be noted. The o_λ numbers are real numbers. The a_λ numbers

[5] Ibid., pp. 404–5.

satisfy the equation $\sum_{\lambda} |a_\lambda|^2 = 1$. This last fact we shall call Theorem 5.[6]

We must admit that our method of acting as if we do not know what we do know has not done so badly by us. Actually, its full potentialities have hardly been touched. But let us be satisfied with what we have, cast our simulated ignorance aside, and consider again what we do know. In other words, let us leave the seclusion of our study and return with our abstract system function F to the concrete world of the physicist's laboratory.

Can we find any factors in the latter world of observation which can be identified with the factors in our abstract system function F? Let us look around.

The world of physics contains certain observed systems; our abstract system function contains certain entities. Suppose we identify the two. This transforms the abstract system function F into the concrete physical system F^P. As a result, the entities s of F become the "systems" which physicists refer to as molar masses, electrons, protons, photons, etc. Similarly, the properties o of s become the physical properties, such as coördinate position, momentum, angular momentum, and energy of physical systems. Quantum physicists call these properties o, observables.

Postulate I of our abstract system informs us that to every "observable" or physical property o of any system s there is a corresponding operator O. Hence, if the world of experimental physics provides an interpretaton of our system function F there should be operators corresponding to the properties of physical systems. This happens to be the case. The position property q has a corresponding coördinate operator Q, the momentum property p a corresponding momentum operator P, the angular momentum property m a corresponding angular momentum

[6] Strictly speaking, this theorem is not true unless certain additional assumptions are made. Great aid in concentrating attention upon the crucial factors, and no weakening of our final conclusion results from our omission of such technical details, for, as the reader will see in the end, our conclusions would be reinforced rather than weakened were these additional assumptions included.

operator M, and the energy property e a corresponding Hamiltonian, or energy, operator H.[7]

It remains to identify our abstract system function F′ which formally defines the functions ϕ or ψ. Our abstract system function F informs us that the ϕ or ψ functions are operated on by the operator O. In the world of physics it is permissible to assume that the element upon which the energy operator operates is the state of the physical system in question. Hence, the functions ϕ or ψ become identified with the physical states of physical systems, and are called state- or eigen-functions. Similar considerations will show that the system function F′ formally defines the extremely general conditions imposed upon the function ϕ, the arguments of which are to be identified with the coördinates of what the physicists term configuration space.

It appears that there is an exact correlation between our abstract system functions F and F′ and the states of physical systems and the configuration space of the more concrete world of experimental physics. Does this permit us to say that our theory has been verified?

In one sense the answer is in the affirmative. Certainly, perceived factors correspond to the abstract factors in our system function. But in another sense the answer must be in the negative. So far, the correspondence is purely qualitative. A theory does not meet the requirements of experimental physics unless the correspondence is also quantitative. To satisfy this requirement the theory must (1) imply certain numbers which (2) may be identified with quantities whose presence can be determined by actual operations of measuring.

Does our abstract system function provide us with numbers? We know that it does. In fact, it somewhat embarrasses us in this respect for it provides us with two sets of numbers, the o_λ s and the a_λ s.

Can either of these sets be identified with the theoretical measurables to which operations of measuring refer? A simple consideration causes us to select the o_λ set. The theoretical measurables to which operations of measuring refer must be real

[7] Ibid., p. 405.

numbers; our o_λ numbers are real numbers. Hence, we identify the two. As a result the o_λ numbers, which the pure mathematician calls characteristic numbers, become identical with the theoretical measurables which the physicist calls eigenvalues.

The identification of the a_λ numbers is not so obvious. However, Theorem 5 of our abstract system function gives a clue. This Theorem informs us that the sum of the squares of all the a_λ numbers is equal to unity. What customary things in the world of experimental physics have this property of summing up to equal unity? A little reflection suggests that probabilities behave in this way. The suggestion arises, therefore, that the a_λ numbers are to be identified through their squares with probabilities. Experience has shown this surmise to be correct.

When these identifications between the aggregate of o_λ numbers and the theoretical measurables and between the $|a_\lambda|^2$ numbers and probabilities are made, it happens that the concept of physical state takes on an entirely new meaning in physics. It is in this consequence of our system function F that the unique character of quantum mechanics consists. Instead of defining the state of a physical system, as did Newtonian Mechanics or Gibbs' statistical mechanics, in terms of a single set of theoretical measurables, quantum mechanics defines the state in terms of an aggregate of such quantities each of which has a certain probability of being observed in a given theoretical measurement of the state in question. For example, in Newtonian mechanics the state of a system was empirically defined when six theoretical measurables for position and momentum were prescribed; in Gibbs' statistical mechanics this happened when theoretical measurables for temperature, volume and pressure were given. But in quantum mechanics the definition of state is quite different. Theorem 1 gives not one set of numbers o_1 but an aggregate of such sets o_λ. More concretely, this means that in quantum mechanics a state function for an electron gives, not one sharp set of values for position and momentum, but a whole aggregate of sets of such values. Similarly, Theorems 3 and 5 correlate with each o_λ number, an $|a_\lambda|^2$ number. Put concretely, this means that in addition to the aggregate of sets of theoretical

measurables for position and momentum, the state function for an electron also assigns to each theoretical measurable o_λ in the aggregate, another specific number $|a_\lambda|^2$ which specifies the theoretical probability with which that particular theoretical measurable will be observed if the theory is correct. Conversely, one determines experimentally that an electron is in a given specific state by finding not one set of values for the defining property or properties in question, but by designating an aggregate of sets of such theoretical measurables, together with the theoretical probability with which each is observed.

At last we have found the precise sense in which the concept of theoretical probability enters into quantum mechanics. Our findings may be summarized as follows: First, from the very general assumptions of our abstract system function F two sets of numbers o_λ and a_λ can be derived which are in correspondence. Second, when the elements s in the system function F are identified with physical systems and the function ϕ is identified with the physical state of any one of these systems, then the o_λ numbers define an aggregate of sets of predicted theoretical measurables for the state in question and the $|a_\lambda|^2$ numbers define the theoretical probability with which the corresponding o_λ numbers will be theoretically observed. In other words the two sets of numbers o_λ and $|a_\lambda|^2$ in correspondence define a theoretical probability aggregate[8] which uniquely defines the physical state of the physical system in question.

It may be added that this physical theory predicts every verified theoretical measurable predicted by previous theories together with important new ones in addition while escaping the predicament in which traditional theories involved us.

We are now able to appreciate the importance of the scientific

[8] I.e., an aggregate of theoretical measurables each with its correlated probability. This aggregate composed of theoretical measurables must not be confused with the quite different aggregate composed of measurements to which it is related in the a posteriori definition of probability. Since only the former type of aggregate enters into this paper it is not necessary for us to use some other word than aggregate to designate one of these two classes of things, as von Mises and Lindsay and Margenau have done. It happens that they have reserved the expression "probability aggregate" for the class of measurements, and use the expression "probability distribution" for what we have termed a "theoretical probability aggregate."

method of acting as if we do not know what we do know. It reduces to a minimum the number of physical assumptions necessary to give the maximum number of quantitative predictions for the subject-matter in question. Difficulties arose in traditional physics because physicists insisted upon conceiving of perceived physical phenomena in certain concrete ways. The result was not merely a contradiction in our physical theory, but also an inability to account for the quantitative measurables actually observed. By acting as if we do not know what we do know we freed ourselves from accidental characteristics of what we do know, and from physical conceptions holding only for a restricted range of phenomena, and, thereby, made it possible to pass to the realm of abstract logical and mathematical forms where we found definite conceptions with the generality sufficient to account for all observed phenomena without the previous contradictions. Such is the method and the achievement of quantum mechanics.

PHILOSOPHICAL CONSEQUENCES

Our analysis has indicated the fundamental assumptions of quantum mechanics and the precise manner in which these assumptions lead to its concept of theoretical probability. The essential point to remember is that the theory generates for any physical system a theoretical probability aggregate which uniquely defines the state of that physical system. We are now in a position to answer our fundamental question: What is the philosophical significance of the concept of theoretical probability in quantum mechanics? Space permits consideration of but two points: (1) Its bearing on the status of causality and (2) its presuppositions.

In discussing the scientific concept of causality it is necessary to define one's terms. Historically, three different factors have entered in part or whole into this concept. They are (1) the state of the physical system at a given time, (2) the manner of defining this state, and (3) the relationship between states at different times. In quantum mechanics, item (1) is given by the general state function ϕ as restricted by the system function F',

item (2) by the theoretical probability aggregate identified with a given state function ϕ, and item (3) by the Schrödinger time equation.[9]

Any definition of causality involves the notion of state and of relationship between states at different times. Certain definitions also prescribe the precise manner in which a state must be defined. LaPlace's famous definition is of the latter type. It asserts not merely that a uniquely defined state at one time uniquely determines a uniquely defined state at any later time, but also that a unique state must be defined solely in terms of one sharp set of numbers for the position and velocity of the mass particles constituting the system.

If this is what we mean by causality there can be no doubt that quantum mechanics has rejected the concept. For it does not insist that only coördinate and momentum properties are permissible in defining the state of a system. Also, as we have indicated, it does not define a state for any property in terms of a single set of predicted measurable quantities; instead it uses a theoretical probability aggregate of such measurables.

But we may well ask whether the particular way in which one happens to define the state of a given system is essential to the concept of causality. Whether a state is defined, as it is in Newtonian mechanics, in terms of a single value for position and momentum, or, as it is in Gibbs' statistical mechanics, in terms of a single value for temperature, pressure, and volume, or, as it is in quantum mechanics, in terms of a single probability aggregate for any sufficient properties, may be said to be irrelevant so far as causality is concerned. The essential notions in this concept are (1) that the state at a given time be uniquely defined in some manner which appeals to measurable quantities, and (2) that given such a uniquely defined state at one time, the theory predicts one and only one similarly defined state at any later time with absolute and unqualified exactness and certainty. Certainly, quantum theory requires this conception, for it introduces many operations, all on an equal footing, with no justification for giving the position-velocity operations a privileged position.

[9] Ibid., p. 470.

It becomes necessary, therefore, to strip the scientific definition of causality from the accidents with which its formulation in conjunction with Newtonian mechanics associated it. The following definition results: A given theory in physics introduces the concept of causality when it permits the definition of a unique state function at a given time, and given such a state function predicts with exactness and absolute certainty a unique state function for any later time. In other words, given uniquely defined states, whenever the relationship between states at different times is necessary and exact we shall say that the law expressing this relationship is a causal law; and whenever it expresses merely the theoretical probability that the later state will occur, it is a non-causal or statistical law.

As Margenau has indicated,[10] when one judges quantum mechanics by this definition of causality, an unexpected conclusion follows: Quantum mechanics retains the concept of causality in physical science. This becomes evident when one examines the Schrödinger time equation which defines the relationship between the states of a physical system at different times. This equation prescribes that given a uniquely defined state function ϕ at one time, a uniquely-defined state function ϕ' at any specified later time is exactly and absolutely determined. We have previously indicated that the measurements of an aggregate of quantities which are observed to occur, each with specified frequencies, uniquely define a state function. Thus the two requirements for causality are satisfied by quantum mechanics.

It will clarify the present status of this concept in physical science if we compare Newtonian mechanics, Gibbs' statistical mechanics and quantum mechanics. In such a comparison three factors are significant: First, the physical properties in terms of which the state of a system is defined. Second, the presence or absence of theoretical probability in defining the state of a system. Third, the presence or absence of theoretical probability in defining the law relating states at different times. With reference to the first factor, Newtonian mechanics permitted only position

[10] Philosophy of Science, 1934, pp. 133 ff.

and velocity; Gibbs' statistical mechanics emphasized volume, temperature and pressure, and quantum mechanics permits the use of any properties which are sufficient. With reference to the second factor, only quantum mechanics uses theoretical probability in the definition of a unique state of a system. In other words, quantum mechanics defines the state of a system uniquely in terms of a theoretical probability aggregate of measurable quantities, whereas both Newtonian mechanics and Gibbs' statistical mechanics require a single set of quantities. With reference to the third factor, only Gibbs' statistical mechanics uses the concept of theoretical probability in the definition of the law relating states at different times. Both Newtonian mechanics and quantum mechanics reject theoretical probability in defining the time relation between states. It appears, therefore, that the key to the presence of causality in science is not, as so many contemporary scientists and philosophers have supposed, the mere use of the concept of theoretical probability, but its use in the definition of the law relating states at different times. Hence, Gibbs' statistical mechanics rejected causal law by introducing theoretical probability into the time equation, whereas quantum mechanics brought causality back into physics by shifting the concept of theoretical probability from the definition of the law relating states at different times to the definition of a unique state at any one time.

It may be noted that the status of causality in quantum mechanics is in no way changed if one considers groups of particles instead of a single particle. This merely expands quantum dynamics for a single particle into quantum statistics. In both cases the state of the system is uniquely defined, and the law relating states at different times holds exactly and without any qualification.

However, the transition from quantum dynamics for a single particle to quantum statistics does affect the theoretical probability aggregate. Up to this point we have introduced the minimum number of assumptions necessary to indicate how theoretical probability enters into quantum mechanics. These assumptions also give Theorem 4 of which the famous Heisenberg

uncertainty principle is a special case. If no additional assumptions are introduced it happens when we pass to quantum statistics that the probability aggregate is the one defined by the classical Maxwell-Boltzmann distribution law.

It can be shown, however, that a state function thus defined will not predict correctly for neutral atoms and photons. When the postulates of quantum mechanics are modified to care for this discrepancy the result is an entirely different probability aggregate defined by the Bose-Einstein statistics.

In similar fashion it can be shown that these assumptions will not provide for the observed behaviour of charged particles. In order to remove this deficiency the Pauli exclusion principle must be assumed as a postulate. The result is a new theoretical probability aggregate defined by Fermi-Dirac statistics.

These additional technicalities have been introduced in order to establish the second and major conclusion of this paper: the concept of theoretical probability has no scientific meaning by itself, and takes on such meaning for the scientific theory in which it occurs only if the concepts and categorical principles, many in number, used in its definition, are assumed to hold exactly and without variation. This is evidenced in the changes which occur in the theoretical probability aggregate, from the Maxwell-Boltzmann through the Bose-Einstein to the Fermi-Dirac statistics, with modification in the basic postulates of quantum mechanics. It is shown also in the different meanings which theoretical probability has in Gibbs' statistical mechanics and quantum mechanics.

The philosophical significance of this conclusion is indicated by one consideration. It has been customary since Hume's attack upon causality and upon relations or laws expressing necessary connections in nature, for certain students to suppose that it is possible to reconcile Hume's empirical philosophy with the existence of science by recourse to the scientific concept of theoretical probability. Our study of quantum mechanics shows that this is a mistake. No escape from the need for propositions expressing causal or necessary connections between natural factors is to be found by an appeal to the scientific concept of prob-

ability; instead, if anything, one merely increases the number of universal propositions expressing necessary connections which must be assumed to hold absolutely and exactly. Otherwise, in quantum mechanics for example, the probability aggregate would be in such a state of flux that no prediction could be made, and the theory would be utterly useless in science.

In order that there may be no ambiguity upon this point, let us indicate the universal, exact propositions asserting necessary connections, which we have shown to be the very minimum necessary to define the concept of theoretical probability as it appears in quantum mechanics. First the one-one correlation between a physical property o and its operator O, which is expressed in Postulate I, must hold without exception. Second, all the identifications between physical factors and formal factors which we have made must be assumed to hold without exception. If the assumptions necessary to guarantee these identifications were stated in the form of postulates at least two more postulates would be added to our system.[11] Third, all the propositions necessary to define the system function F′ must hold with equal rigidity. These propositions are not inconsiderable in importance or in number. In the first place, they must include all those necessary and sufficient to define Euclidian geometry for the configuration space. In the second place, the law that the number of dimensions in configuration space is equal to the number of degrees of freedom of the physical system considered must also hold. Finally, the Schrödinger time-equation expressing a causal relation between states at different times must hold categorically. If, in addition, one wants to provide for photons, electron-spin, etc., then additional similar propositions, such as the Pauli principle, are necessary. It appears, therefore, that the person who hopes to reconcile the principles of Hume's philosophy with the existence of science by recourse to the concept of probability would do well to stay away from quantum mechanics.

The situation is no better if one appeals to Gibbs' statistical mechanics in which the causal type of law relating physical

[11] See Lindsay and Margenau, pp. 404–411.

states at different times is replaced by a law of the statistical, non-causal type. For the number of propositions asserting necessary connections required to define theoretical probability is just as great whether one uses theoretical probability to define the time relation between state functions, as Gibbs' mechanics does, or to define a single unique state function, as quantum mechanics does. Hence, a general rule concerning the universality of statistical laws in nature can be stated. This rule is that if there are certain laws in science which are statistical then there must also be laws in that science which are not statistical. Otherwise the concept of theoretical probability essential to the meaning of the statistical law in question cannot be defined.

Our conclusions may now be summarized. First, a distinction must be drawn between the numbers predicted by a specific scientific theory, and the numbers gained by the actual operations of measuring in verifying that theory. The former numbers we call theoretical measurables, the latter, operative measurables. Second, theoretical measurables and operative measurables are never identical, even when scientific theories are said to be verified. This lack of identity is in kind as well as quantity. For, as we have indicated, the theoretical measurables predicted by quantum theory are real numbers, whereas the operative measurables given by actual operations of measuring are always natural numbers. Moreover, the real numbers which are the theoretical measurables cannot be defined in terms of the natural numbers which are given by operations of measuring since actual operations of measuring give only a finite number of natural numbers in any given case, and it is impossible to follow Dedekind and Cantor's method of defining real numbers in terms of classes of rationals which are in turn defined in terms of classes of natural numbers unless one has an infinite number of naturals. It appears, therefore, even for the relatively few concepts in any given scientific theory which are associated directly with operations, that the operational theory of the scientific concept gains what plausibility it has, only because its proponents overlook the fundamental distinction between theoretical measurables and operative measurables. Third, a similar distinction must be

drawn between probability referring to theoretical measurables and probability referring to operative measurables. The former type of probability we call theoretical probability, the latter type operative probability. Fourth, quantum mechanics, qua quantum mechanics, refers only to theoretical measurables and to theoretical probability. Operative measurables and operative probability are the subject-matter of the theory of errors, and are associated with each specific theoretical measurable in the aggregate of theoretical measurables of quantum theory, in exactly the same way, as they are associated with the specific theoretical measurables of any other scientific theory. Fifth, the abstract assumptions of a certain system function F generate two sets of numbers o_λ and a_λ in correspondence, which in quantum physics constitute a theoretical probability aggregate that uniquely defines the state of a physical system. Sixth, this use of theoretical probability in the definition of the state of a physical system, instead of in the definition of the time relation between states, permits quantum mechanics to retain the concept of causality. Seventh, the concept of theoretical probability, as actually used in physics, has no meaning by itself and presupposes in its definition a large number of principles expressing necessary connections which must be assumed to hold exactly. Hence, the existence of statistical laws is impossible without the existence of non-statistical laws.

It appears, therefore, that one solves no philosophical problems concerning the status of necessary connections in science by appealing to the concept of theoretical probability. Instead, when one replaces a proposition asserting a necessary connection with one asserting merely a probable connection, one introduces more assumptions concerning necessary connections than would have been required to state a necessary relationship in the first place. Apparently, the reason for this is that the concept of probability is an exceedingly sensitive concept because of its necessary definition in terms of a very large number of assumptions in any given scientific theory; hence, unless these assumptions hold exactly and constantly, the concept of theoretical probability becomes so fluid as to be scientifically useless.

It seems evident, therefore, that the time has come for philosophers to say less about the concept of probability and to give their attention instead to the philosophical assumptions required to validate the many necessary principles which it presupposes in a given verified scientific theory, such as quantum mechanics, in which it occurs.

1891 Yale Station,
New Haven, Conn.

DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

THE VALUE OF THE INEXACT

Sir,

The subject of chemical concepts as opposed to physical ones has always been fascinating to me because it shows the great value of inexact ideas. It is easy to prove that no completely exact statement can be of any value in natural science, but when applied to physics the argument always appears to be a combination of far-fetched trivialities and sophistry. Of course, the mere fact that there is no absolute security for the validity of what we consider exact natural laws should lead to the conclusion that these laws are only valuable in combination with the element of uncertainty in them, which is compensated by the supreme sanction of validity, which is faith.

This, however, shows itself in a much more matter-of-fact fashion when we consider chemical concepts. Chemistry is a world of ideas expressed by such terms as 'relative stability,' 'affinity,' 'tendency,' 'inclination,' 'general expectation,' as descriptions of behavior. There is not a single rule in chemistry which is not qualified by important exceptions. The character of a substance or class of substances is as complex as the features of physiognomy and the art of chemistry appears to be the power of being aware of these complex attitudes of matter, and in a thousand delicate attempts to add further evidence to, and enlarge the field of this awareness; thus, were a million compounds synthesized it would be an achievement which could never have been attained by exact methods. It is indeed obvious, that if at any time chemists would have been so ill-advised as to let themselves be frightened by physicists into abandoning all vague methods, and to restrict themselves to the field where exact laws (or what are supposed to be such by the physicists) pertain, the development of chemistry, would, at that

moment have stopped dead, and its most valuable parts would have melted away in the rays of such foolish criticism.

I think it is good to contemplate how useless, or even harmful exactitude becomes at so close quarters to physics. Just link up two of three of the atoms of physics, and their behavior becomes so complex as to be beyond the range of exactitude. How supremely unreasonable it appears then, to claim that, by precise measurements and mathematical treatment, i.e. physical exactitude, a vital knowledge and command of such objects as living organisms and social bodies should be found. All these fields of high complexity gain real profit only from the discovery of specific tendencies of behavior incorporated in their functional outlines.

Chemistry, indeed, leads us so far away from physics, (or let us say, that physics appears, when we look at chemistry, so far remote from everything else in the world) that the description of chemical substances and the art of dealing with them lies quite near, by comparison, to the types of human behavior and the art of commanding human behavior. The mythological language of the alchemists persists in chemistry and is still characteristic of its most vital element.

M. Polanyi.

University of Manchester.

IMMUNITY OF THE APRIORI

Sir:

In his article on "Methodology of Modern Physics" in "Philosophy of Science" for January 1935, Mr. H. Margenau says: "The impressive system of analysis outlined in Kant's 'Critique of Pure Reason' was adequate for the science of his day, but his transcendental esthetics broke down when Einstein discovered the interrelatedness of time and space. More and more of his methodological principles crumbled away in the face of newly discovered facts." These two sentences are so crammed with wrong implications that they should not be allowed to pass unchallenged in any publication on the philosophy of science, even though it devolves on so humble a student of Kant as the present writer to enter the lists. To avoid crowding the margins of the customary letter, however, I can do no more here than to indicate the major misrepresentations.

In the first place, Kant did not outline any system of "analysis," and his studies—in the work mentioned—were not intended to be applied

to, and certainly were not a methodology of, the "science" of his or any other day, if we retain the ordinary meaning of the terms. His work was primarily a critique or study of the a priori foundations of our knowledge, and no facts (which, as the word is ordinarily used, are empirical or a posteriori) either old or newly discovered could cause it to crumble away. Only a criticism of his work showing the errors in his reasoning could do that.

Moreover, so far as I have been able to learn in my limited studies, Einstein did not discover the "interrelatedness of time and space." Kant himself, of course, did point out a very close and fundamental relationship between them, designating them as the two forms of our intuition, but even he did not claim to be the first to suggest that they were related. Whether or not we consider that they are related depends primarily on just what we mean by relationship. If we mean that they may be employed in the same algebraic expression to define some function—such as $ds^2 = -dx^2 - dy^2 - dz^2 + dt^2$—then any observation of velocity gives data on such a relationship, since velocity, in its differential expression as ds/dt, is exactly the relationship between space and time for the object under observation.

In the second edition of the Critique, (1787) Kant remarked in the preface that there was no danger of his work's being refuted, but that there was of its being misunderstood. One need only pick up almost any of the criticisms of Kant to see how right he was in the second part of his statement at least. One thing that has always impressed me about the relativity theory, and led me to feel that it was basically sound, is that its philosophical, or more precisely its epistemological, conclusions were in direct corroboration of Kant.

PHILIP C. JONES.

83 Old Short Hills Road,
Milburn, N. J.

BOOKS RECEIVED

Language, Truth and Logic, by A. J. Ayer. Gollancz. 1936. 9/-.
The Problem of Time. University of California Press. 1935. $2.25.
Philosophical Essays for Alfred North Whitehead. Longmans, Green & Co. 1936. $3.00.

ANNOUNCEMENT

SECOND INTERNATIONAL CONGRESS

The Second International Congress for the Unity of Science will take place in Copenhagen, June 21–26, 1936. The central topic will be the relation of physics and biology (including psychology), with especial emphasis upon the concept of causality. A small number of invited speakers will provide the context for a general discussion. Niels Bohr will participate in the Congress.

Inquiries and notices of intended attendance at the Congress may be directed to the Secretary, Dr. Otto Neurath, Mundaneum Institute, 267 Obrechtstraat, The Hague, The Netherlands. The subscription for active membership is 100 francs.

The Proceedings of the First Congress, held at the Sorbonne in September 1935 and devoted to the general topic of the logic or philosophy of science, are to appear in the early months of 1936 in the form of eight small volumes (Hermann and Co., Paris). A detailed report of the papers and discussions of the Congress appears in volume 5, number 6 of the journal *Erkenntnis* (Felix Meiner, Leipzig). Plans are being made to hold one of the Congresses in the United States within the next few years.